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ABSTRACT

This report on the short-term effects of Upward Bound, a federal pre-college program designed to help economically disadvantaged students complete high school and gain access to post-secondary education, presents interim findings from the Longitudinal Effectiveness Study of Upward Bound based on data on approximately 2,800 students during the first year or two of high school. At present, there are more than 600 Upward Bound projects; they offer intensive instructional programs and are usually hosted by 2-year and 4-year colleges. The study found that: (1) Upward Bound has early positive impacts on students' educational expectations and academic course-taking; (2) students with lower educational expectations initially benefit more from Upward Bound; (3) Hispanic students initially benefit most from Upward Bound; and (4) many students (about 37 percent) who enter Upward Bound leave the program during the first year. After an executive summary and introductory chapter, Chapter 2 presents data on persistence in Upward Bound, and on the Upward Bound services offered. Chapter 3 details short-term impacts of Upward Bound, including the average impact of the program and groups benefitting most. The concluding chapter summarizes findings, compares them to previous findings, and draws implications for program improvement. Eight appendices provide additional detail on research methodology, data interpretation, and statistics. (Contains 18 references.) (DB)

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May 1997

The National Evaluation of Upward Bound

The Short-Term Impact of Upward Bound: An Interim Report

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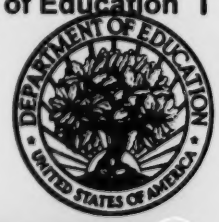

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EXECUTIVE SUMMARY

Upward Bound, which was initiated in 1965 as part of the War on Poverty, is a federal precollege program designed to help economically disadvantaged students complete high school and gain access to postsecondary education. Besides federal student-aid programs for college students, Upward Bound represents the largest federal intervention that helps students attain a postsecondary education. At present, there are more than 600 Upward Bound projects serving 42,000 students. Federal requirements mandate that at least two-thirds of each project's participants must be from households that have low income (less than 150 percent of the poverty threshold) and where neither parent has graduated from college; up to one-third of the participants can be from either low-income or first-generation college families.[1] Upward Bound projects offer intensive instructional programs that also include tutoring and counseling services. Students meet throughout the academic year with Upward Bound staff and generally participate in an intensive summer program that lasts about five to eight weeks. Most students enter the program for the first time while in 9th or 10th grade and some students remain with Upward Bound through 12th grade. Upward Bound projects are usually hosted by a two- or four-year college. Some Upward Bound projects are hosted by community-based organizations and high schools. In 1996, the annual average cost per participant was about $3,800.[2]

This report contains findings from the Longitudinal Effectiveness Study of Upward Bound conducted by Mathematica Policy Research, Inc. (MPR) and its subcontractors, Westat and Decision Information Resources, under contract to the U.S. Department of Education (ED), Planning and Evaluation Service. This represents the first large-scale evaluation of the Upward Bound program since Research Triangle Institute's evaluation nearly twenty years ago.

Based on data for more than 2,800 students, this report describes the short-term impact of Upward Bound on students during the first year or two of high school. A second report, using data drawn from a survey of students that will be completed in late 1997, will describe the program's impact on outcomes such as high school graduation and college enrollment and persistence. The research questions addressed in this report include:

- How long do participants stay in the program? How many participants drop out of Upward Bound, and what are their reasons for leaving the program? What kinds of services do participants receive from Upward Bound projects?

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[1]We find that 80 percent of new applicants are from both low-income and first-generation college families, almost a fifth are from first-generation families only, and less than 5 percent are from low-income families only.

[2]A detailed description of Upward Bound projects can be found in Fasciano and Jacobson (1995) and a description of Upward Bound Target Schools can be found in Waldman et al. (1995). An overview of Upward Bound that draws on numerous sources can be found in Moore (1997).

- What is the impact of Upward Bound on students' education-related outcomes? Do some students benefit more than others from participation in Upward Bound?

About the Longitudinal Effectiveness Study

The longitudinal effectiveness study is part of a comprehensive five-year evaluation of Upward Bound projects, target schools, and students being conducted by MPR and its subcontractors. The study focuses on *regular* Upward Bound projects, which means it excludes projects that serve veterans or those that received special grants to emphasize a math/science curriculum. In addition to the longitudinal effectiveness study, the national evaluation includes a survey of nearly 250 Upward Bound project directors, detailed field visits of 20 Upward Bound projects, and a survey of more than 700 Upward Bound target schools.

The findings in this report are based on several sources of data. First, as Upward Bound projects recruited students to fill program slots during 1992-94, eligible applicants were asked to complete a baseline questionnaire that asked about their family background, attitudes and expectations, and school experiences. Second, during the spring and early summer of 1994, we asked these applicants to complete a follow-up survey so that we could update their school-related experiences, attitudes, and expectations. More than 97 percent of the program applicants responded to the survey. In addition to the survey data, we used data obtained from high school transcripts collected after the 1994 academic year, and reports of students' participation in Upward Bound services that were recorded by Upward Bound project staff. Students were surveyed again in summer 1996 and the study team collected students' high school and postsecondary transcripts after the 1996 school year. This last round of data collection provided a more complete picture of students' high school experiences, academic preparation, college plans, and for some students, college enrollment and persistence.

To assess program impacts, the evaluation selected a nationally representative sample of 67 Upward Bound projects hosted by two- and four-year colleges. Eligible applicants in the 67 Upward Bound projects during 1992-94 were randomly assigned to Upward Bound or a control group. The Upward Bound group included 1,524 students and the control group included 1,320 students. Short-term impacts were estimated by comparing students in the two groups on a range of measures including high school performance (for example, grades and course taking), attitudes and expectations, and parental involvement. Using the randomized experimental design along with a nationally representative sample of Upward Bound projects yields program impacts that have both internal and external validity, that is, the design allows us to isolate the impact of Upward Bound on student outcomes and to generalize the findings to all Upward Bound projects hosted by two- and four-year colleges.

Major Findings

The findings contained in this report focus on two related issues: (1) the short-term impact of Upward Bound on students' education-related outcomes and course taking, and (2) students' persistence in the Upward Bound program.

Upward Bound Has Early Positive Impacts on Students' Educational Expectations and Academic Course Taking

Educational Expectations

Evidence obtained from students about the number of years of schooling they plan to complete shows that, on average, Upward Bound participants expected to complete almost .25 more years of school than did members of the control group. Both Upward Bound participants and students in the control group typically experienced a decline in expectations between the time of application and the follow-up survey; however, the decline was larger for the control group. This finding suggests that involvement in the program was more likely to reinforce participants' high expectations, while no involvement allowed the control group's expectations to erode between the time of application and the follow-up survey.

Upward Bound also had a positive impact on parents' educational expectations as reported by students: parents of Upward Bound participants expected their children to complete about .3 more years of schooling than did the parents of children in the control group. Little change was observed in the expectations of participants' parents; however, relatively large declines in expectations were observed for parents of children in the control group.

Course Credits

Besides having a positive impact on educational expectations, Upward Bound increased the number of credits students earned during the first few years of high school: program participants earned about 1 credit (Carnegie unit) more than nonparticipants. This overall impact of 1 credit stems from participants earning substantially more credits in science, math, English, foreign languages, and social studies. Participants also earned more credits than nonparticipants in vocational education courses and in remedial math courses. When these impacts are compared with the experiences of a typical high school student who each year is expected to complete about five academic and/or elective credits, they appear quite large.

Students with Lower Educational Expectations Initially Benefit More from Upward Bound

Educational Expectations

Although almost three-quarters of the eligible applicants expected to complete at least a four-year college degree before entering the program, students who benefitted most from participating in Upward Bound were those with lower expectations (that is, students who did not expect to complete a four-year college degree.) Upward Bound increased parents' educational expectations to a greater degree when students started Upward Bound with low expectations. Upward Bound increased fathers' expectations by 1.2 years for participants who did not expect to complete four years of college. Neither fathers' nor mothers' expectations for participants who held higher expectations changed and no difference was observed for changes in students' own expectations regardless of their initial plans.

Course Credits

In terms of outcomes related to academic preparation, Upward Bound had larger positive impacts on credits earned in math, English, and social studies for students with lower expectations. These students gained almost .6 more math credits than members of the control group; the corresponding figure for students with higher expectations is .1 credit. In addition, participation in Upward Bound led to an increase of about .8 credits in English and social studies for students with lower expectations. Across all academic subjects, Upward Bound increased the number of credits earned by 3.1 for participants with lower expectations and by .5 credits for those with higher expectations.

Hispanic Students Initially Benefit Most from Upward Bound

Among the three largest race/ethnic groups in Upward Bound, a consistent picture of program impacts on course taking emerged: Hispanic students routinely experienced larger returns from participating in Upward Bound than either African American or white participants. Hispanic students gained more than two credits, while African American and white students in Upward Bound gained less than .5 credits. Higher returns to participating in Upward Bound for Hispanics are obvious in several subjects: math, English, foreign languages, social studies, and vocational education.

Many Students Who Enter Upward Bound Leave the Program During the First Year

While the short-term impacts suggest that Upward Bound has a substantial effect on educational expectations and course taking, the effect may have been even larger if more students stayed in the program. Even in the first year, participants who leave Upward Bound early, for example, do not earn as many credits in high school as those who remain. Despite the value of staying, many students do choose to leave Upward Bound in the first year. Projections based on the experience of all students in the study suggest that 37 percent of those who participated will leave within the first 12 months. This program dropout rate is very likely to increase at the close of the junior year--when project staff say that students are most likely to leave the program for jobs. All told, attrition from Upward Bound may be quite substantial by the time students finish high school.

I. INTRODUCTION

Upward Bound, along with Head Start and Job Corps, was the War on Poverty's major response to the dismal educational opportunities faced by many low-income students. The goal of this program is to provide supplemental academic and support services to students from disadvantaged backgrounds in order to help them finish high school, gain admission to college, and successfully complete a degree program. Since its first year of full funding in 1966, the program has more than doubled in size, now serving 42,000 students per year.

In December 1991, the U.S. Department of Education funded a five-year longitudinal evaluation of Upward Bound. The objectives of the evaluation include documenting how Upward Bound projects deliver services to program participants, the kinds of services offered, the role of target schools in the Upward Bound program, and the impact of Upward Bound on participants' education-related outcomes. This report presents estimates of the short-term impacts of the program, based on data collected during the period 1992-94. These data allow us to assess the *short-term impacts* of the program. A final report to be submitted to the U.S. Department of Education in late 1997 will describe the longer term impacts of Upward Bound.

This introductory chapter reviews the program and policy background for the study, and describes the policy context and design. Chapter II examines students' persistence in Upward Bound, the reasons students report for leaving the program, and the range and intensity of the services they receive. Chapter III presents estimates of the short-term impact of Upward Bound on students' education-related outcomes, including educational expectations, attitudes, academic preparation and grades, behavior in school, and parents' involvement in school-related activities.

A. PROGRAM AND POLICY BACKGROUND

Upward Bound projects offer intensive academic, tutoring, and counseling services to youth who are between 13 and 19 years old and in grades 9 to 12. Two-thirds of the Upward Bound participants in each project must be potential first-generation college students whose families have incomes at or below 150 percent of the poverty threshold; the remainder must meet either the low-income or the first-generation requirement. Projects are required to provide five to eight weeks of services to students each summer and to reinforce the summer experience with at least weekly services during the school year. The summer component is usually designed to simulate a college experience and often involves full-time residence on a college campus. During the academic year, participants can meet with counselors and instructors after school or on weekends. Estimates of the cost of these services show that the per-pupil cost in 1996 was $3,800.[1,2] At present, there are more than 500 regular Upward Bound projects, each serving 50 to 150 students with an average of 90. About 68 percent of the projects in 1995 were hosted by four-year colleges, 28 percent were hosted by two-year colleges, and 4 percent were hosted by community-based organizations and high schools.

The need for programs such as Upward Bound seems as apparent today as it was more than 25 years ago. In the mid-1960s, Coleman and others documented the poor academic preparation and low educational attainment of economically disadvantaged youth (see, for example, Coleman et al. 1966, Jencks et al. 1972, Mosteller and Moynihan 1972). Since the call to arms in *A Nation at Risk* (National Commission on Excellence in Education 1983), the nation's schools and educators have attempted to rectify this situation and improve the academic preparation of all students. However,

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[1]See Fasciano and Jacobson (1995) and Moore (1997) for a comprehensive description of Upward Bound grantees.

[2]In contrast to the intensive and costly services provided through Upward Bound, the federal Talent Search program spends $230 dollars per student, and the federal Student Support Services program's per student cost is $800.

after more than a decade, we still find a large gap in academic performance between students from economically disadvantaged and advantaged families. Recent analyses published by the National Center for Education Statistics suggest, for example, that for the past 20 years, the gap in reading and math achievement between students from families with high and low socioeconomic status (SES) has remained large or may have increased (Green et al. 1995). Data from the National Assessment of Educational Progress suggests a slight narrowing of the achievement gap for whites and blacks, but a substantial gap still persists (NAEP 1994). Although enrollment in vocational and general high school tracks has become less common among low-SES students, almost three-quarters of these students still report participating in one of these noncollege preparatory tracks (Green et al. 1995). During the same period there was a small increase in the proportion of high-SES students in these programs; only one-third of the high-SES students participate in a nonacademic high school program. Statistics on college-going behavior suggest some improvement for low-income students enrolling in postsecondary schools, but there remains a large gap between these students and their high-income counterparts. For example, there is a full 30 percentage point difference between high school graduates from low-income families enrolling in college and high-income students who go to college (Mortenson 1995). Statistics from the mid-1980s also show that if the gap in academic achievement were closed for blacks and whites, both groups would have similar college enrollment patterns (Myers 1987).[3]

[3]When estimating the effect of achievement on the college attendance gap, Myers used statistical adjustments to hold constant other background variables including family income and mother's educational attainment.

B. STUDENTS RECEIVING UPWARD BOUND SERVICES

As we already noted, Upward Bound participants must be from either low-income families or families where neither parent completed a four-year college degree.[4] Comparing Upward Bound applicants during the 1992-94 period with other students from similar grades and from families with low socioeconomic status shows that Upward Bound applicants have higher educational attainment expectations, are better prepared academically, and have parents who are generally more involved in their children's school-related activities.[5] Upward Bound applicants are also disproportionately made up of young women and African American students.

- ***Educational expectations***. Upward Bound applicants have higher educational expectations than other high school students and students from families with low socioeconomic status (SES).[6] Three-quarters of Upward Bound applicants report that they expect to complete at least a four-year college degree, while three-fifths of all students and about one-third of low-SES students report similar expectations.

- ***Course taking and grades***. Upward Bound applicants have better academic preparation than other low-SES students. About 40 percent of all applicants had completed at least one geometry course before entering Upward Bound. Looking at all students in the same grades we find similar results; however, low-SES students are less likely to have taken geometry at the same point in school (27 percent). About 25 percent of applicants had completed two or more foreign language courses before entering Upward Bound. In contrast, 17 percent of low SES students had completed two or more courses. About 45 percent of the applicants have a 9th-grade GPA below

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[4]An overview of the characteristics of Upward Bound applicants is shown in Technical Appendix F.

[5]The comparison is based on students who participated in the 1988 National Educational Longitudinal Survey of Eighth Graders (Ingels et al. 1994).

[6]Students with low socioeconomic status (SES) are in the lower third of the SES distribution. SES is defined in terms of family income and parents' education and occupation. The items are weighted and then summed to form Duncan's Socioeconomic Index. The weights are described in Nakao and Treas (1992).

a C; about 37 percent have a GPA of a C or B.[7] The average 9th-grade GPA for all applicants is 2.4 (C+).

- ***High school graduation requirements***. Many Upward Bound applicants are meeting academic course requirements before entering Upward Bound. On average, states require students to complete about 13 course credits in English, social studies, foreign languages, math, and science before they can graduate from high school. This means that over a four-year period students should complete about three courses from these subject areas each year. Among applicants who are in grades 10 or above when entering Upward Bound, we find that 50 percent completed at least three courses in these subject areas in 9th grade and 75 percent completed two or more courses.

- ***Parental involvement***. Parents of Upward Bound applicants have higher levels of involvement in their children's activities than other parents. More than 40 percent of the applicants report that their parents **often** check their homework compared to 22 percent of all low-SES students who report that their parents **often** check their homework. About 26 percent of all students in the same grades as the Upward Bound applicants report their parents **often** check their work.

- ***Background characteristics***. Compared with high school seniors, Upward Bound applicants are disproportionately female (70 percent) and African American (53 percent); about 20 percent are white, another 20 percent are Hispanic, and about 7 percent are Asian or Native American. In comparison, about 50 percent of all high school seniors in 1992 were boys, 12 percent were African American, 73 percent were white, almost 5 percent were Asian, and 10 percent were Hispanic.[8]

C. THE RESEARCH CONTEXT

The Upward Bound program has been the subject of many evaluations sponsored by the U.S. Department of Education and its predecessor, the U.S. Office of Education. Many of these studies, however, relied on student records from national surveys or administrative records maintained by the Upward Bound program, or they focused on a single Upward Bound project. Only one comprehensive evaluation of Upward Bound's effectiveness has been completed since the program's inception in 1966. Conducted by the Research Triangle Institute (RTI) between 1973 and 1979, the

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[7]We have not been able to identify comparable, published data for all students or other low-SES students in similar grades.

[8]We have not been able to identify similar published data for students from low SES families.

study examined project characteristics and evaluated the effectiveness of Upward Bound for a range of outcomes, including academic preparation and college enrollment and retention (Burkheimer et al. 1979).[9] RTI's basic conclusions about the effectiveness of Upward Bound included the following:

- Upward Bound does not affect students' high school academic preparation or grades.
- Upward Bound has a positive effect on college enrollment.
- Upward Bound has positive effects on overall educational attainment, but no effect on students' persistence in college.

The current evaluation conducted by Mathematica Policy Research (MPR) and its subcontractors, Westat, the Educational Testing Service, Public and Private Ventures, and DRI Inc., presents findings from the national evaluation of Upward Bound's longitudinal impact study. Students included in the study applied for Upward Bound during the 1992-93 and 1993-94 school years during which period we collected baseline data. The first round of follow-up data collection occurred in the summer of 1994, gathering information about students' educational experiences, course-taking patterns and grades, attitudes and expectations, and participation in the Upward Bound program. This first follow-up data collection forms the basis of the findings reported here. Another round of data collection, conducted in 1996 and 1997, will serve as the basis for a detailed assessment of the impact of Upward Bound on students' high school experiences and early college-going behavior.

The following research questions are addressed in this report:

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[9]RTI's approach for examining the effects of Upward Bound focused on a comparison group methodology. Using this method they attempted to identify students from Upward Bound target schools who were similar to participants. While they were able to remove some differences, they found, for example, that members of the comparison group had lower educational expectations than Upward Bound participants. Differences such as this between the Upward Bound sample and the comparison group sample can lead to erroneous conclusions.

- How long do participants stay in the program? How many participants drop out of Upward Bound, and what are their reasons for leaving the program?
- What kinds of services do participants receive from Upward Bound projects?
- What is the average impact of Upward Bound on students' education outcomes? Do some students benefit more than others from participation in Upward Bound?

We also examined whether some project characteristics were associated with differences in persistence and project-specific impacts. Analyses showed that while there was substantial variation among projects for students' persistence in Upward Bound and several education-related outcomes, no clear patterns of associations with project characteristics were discovered. Technical Appendix H shows the distribution of project impacts for selected outcomes.

D. RESEARCH DESIGN

The results described here are based on the first rigorous evaluation of the Upward Bound program in the last 20 years. The research design uses a nationally representative sample of Upward Bound projects and random assignment selection procedures for constructing two statistically equivalent groups of students: a treatment group selected for Upward Bound and a control group. By using a probability sample of projects and random assignment of students to a treatment and control group, this design provides both strong external and internal validity. We can answer the following general question: *On average, how do the educational experiences of Upward Bound applicants and participants compare to what they would have achieved in the absence of Upward Bound?* By analyzing impacts for all students randomly selected for Upward Bound, we can estimate

impacts for applicants. By taking into account how many students showed up once offered a program slot, we can estimate impacts for participants.[10]

From the list of regular Upward Bound projects, we selected 70 projects for the impact study.[11] Eleven of the projects were not able to participate in the study for a variety of reasons. Some were defunded by the U.S. Department of Education during the 1991-92 grants competition. Others did not expect to recruit new students for the 1992-93 school year, or had too few applicants to construct a control group or to fill all funded program slots. We replaced with a randomly selected similar project all but three of the projects that could not participate. The final sample for the longitudinal impact study includes 67 Upward Bound projects.

To assess program impacts, we constructed treatment and control groups from students selected at random from the pool of eligible applicants during the 1992-93 and 1993-94 school years. Eligible applicants were defined as students whom projects recruited and who met both federal requirements and project specific criteria for participation. The treatment group includes 1,481 students, and the control group, 1,266 students. Students in each project were given the same chance of being

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[10]Before we randomly selected students for Upward Bound and the control group, we asked project directors to rate applicants as most likely to be selected, somewhat likely to be selected, and least likely to be selected under normal selection rules. Looking at these three groups of students we discovered few differences in their observable characteristics and few differences in who benefitted most from the Upward Bound program. These findings suggest that random selection of students meeting both the federal criteria and minimum project-specific standards did not substantively alter the kinds of students projects normally serve.

[11]The sample represents Upward Bound projects that are (1) in the 50 states or the District of Columbia; (2) hosted by a postsecondary institution; (3) mature, having operated for at least three years by October 1992; and (4) not dedicated to serving only students with physical disabilities. Eliminated from the sample were new projects, projects hosted by high schools or community-based organizations, projects in the territories, and projects that serve exclusively disabled students. The 70 projects in the impact study also participated in a survey of Upward Bound project directors (for a description of this survey, see Fasciano and Jacobson 1995).

selected for Upward Bound.[12] All projects served the same number of students they would usually serve when normal selection procedures were used. Implementing random assignment in the 67 projects took about 14 months. We used this long build-up period so that projects could use their normal recruiting procedures and enroll students following their regular enrollment schedules. Procedures for drawing the sample and constructing sample weights are described in Technical Appendix A.

The random assignment design allows us to construct estimates of program impacts that are free of selection biases that have plagued other attempts at assessing the effectiveness of Upward Bound.[13] The only difference between the Upward Bound group and the control group is the availability of Upward Bound services.[14] The groups are otherwise statistically equivalent. Using the treatment and control groups, we computed two estimates of program impact for each outcome. The first estimate refers to the impact of Upward Bound for *all* students selected for the treatment and the control group. Thus, the treatment group includes both students who chose to participate once offered a slot and those who did not. The second estimate corresponds to the program's impact on *participants* and adjusts for the fact that some students offered openings declined to participate. Our approach for making adjustments for nonparticipants is described in Technical Appendix C.

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[12]In many cases, projects requested that we maintain a specific mix of boys and girls or select predefined numbers of students from each of their target schools. To accommodate this request, we used a stratified random sampling scheme to ensure the project composition met their requirements.

[13]A detailed statement of the random assignment procedures is presented in Myers et al. (1993) and a comparison of the Upward Bound and control groups at the time of application (and before random assignment) is presented in Technical Appendix B.

[14]Upward Bound project directors provided services to 29 students selected for the control group. While we allowed these students to continue to receive Upward Bound services, for purposes of the analysis we maintained their original status as members of the control group. By maintaining their status, we obtained a trivially small underestimate of the program's impact. To treat these students as members of Upward Bound or to remove them from the analysis sample would violate the random assignment procedures and reduce the study's internal validity.

Data were collected during two periods. First, we collected information from students using a selfassessment completed when they applied to the program (baseline data collection). Second, we collected data from students through a telephone survey in the spring of 1994. For the baseline data collection, we achieved almost a 100 percent response rate, and for the follow-up survey, we achieved a response rate of more than 97 percent. In addition to obtaining information from students, we collected student transcripts that include detailed information on course taking and grades, and we asked Upward Bound project directors to report on the range and intensity of services received by students in the sample. We also supplemented the student data with survey data collected from Upward Bound grantees and the target schools attended by students in our sample.

II. PERSISTENCE IN UPWARD BOUND AND UPWARD BOUND SERVICES

Applicants may take one of several paths after applying for Upward Bound. Some applicants will not be selected by a project because they do not meet project specific criteria, such as grade point average, or because they have disciplinary problems (Fasciano and Jacobson 1995). Other applicants may choose to pursue another program, take a job, or participate in sports. Over 14 months we selected 1,481 students for Upward Bound. This chapter draws on data extracted from administrative records reported by Upward Bound project directors and their staff and data from the first follow-up survey of students to document some of the experiences of these youth. In analyzing the data, we examined the number of applicants who participated in Upward Bound, the number who persisted, and the duration of their involvement in the program. We also explored the reasons students gave for not participating, and we examined the services students received from Upward Bound.

A. PARTICIPATING IN UPWARD BOUND

- Twenty percent of the eligible applicants MPR randomly selected for Upward Bound did not participate when offered a program slot. This no-show rate does not appear to be a product of random selection including students in the program who had higher than normal no-show rates; instead, it reflects the typical experiences of students selected for Upward Bound.
- Some of the reasons given for not participating included taking a job, transportation problems, and family issues.
- Hispanic and Asian students are more likely to participate after being selected than African American students. Younger students are more likely to participate than are older students.

About 20 percent of the students we randomly selected for Upward Bound did not participate; that is, they did not accept an offer to enter Upward Bound.[1] During the first follow-up survey, we asked the nonparticipants to list the reasons behind their decisions. Some reasons included taking a job, transportation problems, family issues, time conflicts, and the program not contacting them. The percentage of applicants reporting each of these reasons is shown in Table II.1. None of the reasons stands out as the dominant justification for not participating and none is frequently given.

TABLE II.1

PERCENTAGE OF NONPARTICIPANTS, BY REASON CITED FOR NOT ATTENDING

Reason	Percentage of Nonparticipants[a]
Took a job	15
Transportation problems	14
Program did not contact student	16
Family issues	11
Time conflict	13

[a]Students could give multiple responses if necessary and percentages do not sum to 100.

[1]We assessed whether nonparticipation was related to the random selection process and projects working with students they might not normally choose. Before we assigned students to Upward Bound and the control group, we asked project directors to rate students as most likely to be selected, somewhat likely to be selected, and less likely to be selected when using normal selection rules. The nonparticipation rates for these three groups are not significantly different. This suggests that even in the absence of the evaluation, about 20 percent of those selected for Upward Bound will choose not to participate.

In addition to looking at the reasons for not participating, we also examined whether specific background characteristics were associated with students' decisions. The factors we examined include sex, race/ethnicity, grade level, low income/first generation status, working for pay during the previous summer, students' educational plans at the time of selection, and project directors' ratings of students' likelihood of being selected for Upward Bound under normal selection rules (see Technical Appendix D for participation rates for selected groups of students). We explored the correlates of participating in Upward Bound by examining the odds (chances) of entering the program while statistically adjusting for differences on other student and family background variables (see Table II.2).[2] An odds ratio of less (greater) than 1 shows that one group of students is less (more) likely to participate than the reference group, net of the other factors. Only two factors have an independent effect on the chances of participating in Upward Bound: (1) race/ethnicity and (2) and students' grade level. Both Asian and Hispanic applicants were more likely to participate in Upward Bound than African American students, and students recruited in later grades were less likely to participate than those recruited earlier. None of the other factors affects participation.

B. PERSISTENCE IN UPWARD BOUND

- Almost 40 percent of new Upward Bound participants drop out of the program within 12 months.
- The most common reason participants drop out of Upward Bound is to take a job.
- Asian and Native American participants persist at a greater rate than African American students. Participants planning to complete college are more likely to remain in Upward Bound than are other participants.

[2]The independent effects of the characteristics are obtained from a binary logit model. Standard errors were computed taking into account the complex sample design.

TABLE II.2

ADJUSTED ODDS OF PARTICIPATING IN UPWARD BOUND

Background Factor	Adjusted Odds Ratio
Female[a]	.81
Hispanic[b]	3.85**
Asian[b]	5.03**
White[b]	1.74
Native American[b]	1.18
Other race[b]	.65
Grade level[c]	.71**
Low income/first generation[d]	1.41
Upward Bound project director's rating	.87
Worked for pay during summer	.84
Plans to complete at least a college degree	1.14

Note: Participants are defined as students who were selected for Upward Bound and started the program.

**Statistically significant at the .05 level.

[a]Reference group is male.
[b]Reference group is African American.
[c]Grade level coded as grade completed the year before applying for Upward Bound (8, 9, 10, 11).
[d]Reference group is low income only and first generation only.

We asked Upward Bound project directors after the 1994 summer program to report when students started participating in Upward Bound, whether they were still in the program, and, if participants left Upward Bound, when they last attended. Using this information, we developed a profile of the chances of Upward Bound *participants* leaving the program monthly (see Figure II.1).[3] Figure II.1 shows the potential experiences for a group of new Upward Bound participants.[4] The figure shows that about 63 percent of participants will remain with the program after the first 12 months, and 37 percent will leave. To see the implications of this, imagine a typical Upward Bound project with 25 new participants--students who decided to participate after being offered an opening by an Upward Bound project. After 12 months, 9 participants will have left the program, and 16 will remain.[5] We can also project this experience for all Upward Bound projects in the nation. Based on the number of students selected for Upward Bound as part of the evaluation, we estimate that nationwide, 17,700 students will enter each year. From among this group, more than 6,500 will leave Upward Bound in the first 12 months, leaving about 11,000 from the initial group. While we cannot

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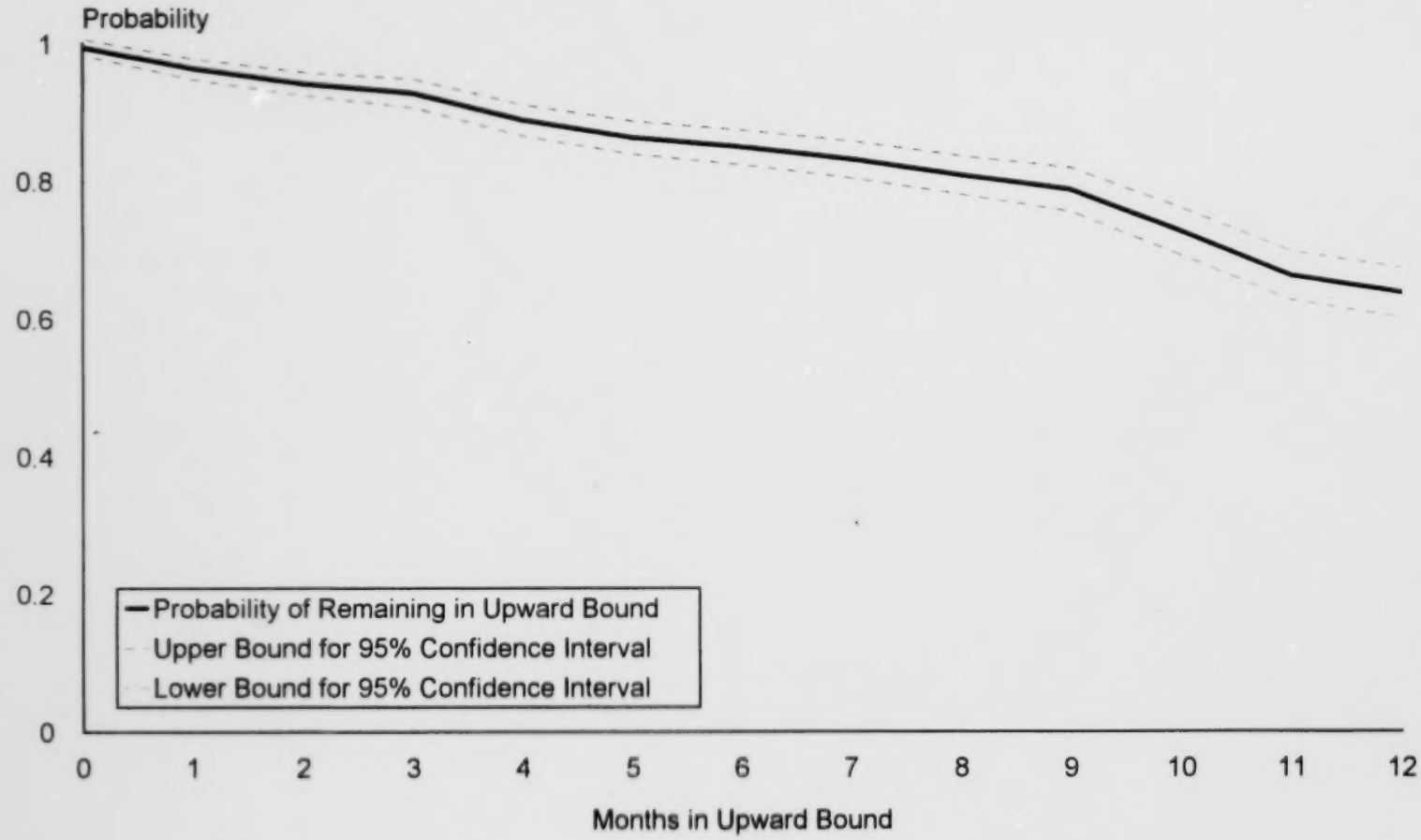
[3]The analysis of program persistence focuses on the experiences of participants--students who showed up for services--and not all students selected for Upward Bound as part of the random selection research design. Estimates presented in Figure II.1 are derived from a hazards model and take into account that not all participants had similar start dates. The confidence intervals are shown so that the reader can assess the precision of the estimates. They show, for example, that the estimated probability of persisting for 12 months is measured with less precision than the probability of persisting for six months. The loss of precision arises because fewer students had an opportunity to participate in Upward Bound for 12 months than six months--a function of the projects using different start dates.

[4]Besides looking at the overall persistence of students in Upward Bound, we also examined the variability in persistence among projects. This analysis showed that while there are considerable differences in persistence among projects, few if any project characteristics were associated with persistence. Some factors we examined included predominant race/ethnicity, size, urban location, type of host institution, and curricular emphasis.

[5]If we include the no-show rate in the computation, then almost half of those *selected* for Upward Bound will have dropped out within 12 months. For example, starting with 25 students selected for Upward Bound we would expect that only 20 will accept a program opening, and only 13 of the initial group will still be participating after 12 months.

FIGURE II.1
PROBABILITY OF REMAINING IN UPWARD BOUND, BY MONTH



directly assess whether all of these students are replaced, anecdotal evidence and data from the grantee survey suggest that many are. For example, when talking with project directors we learned that projects often maintain a waiting list. Also, the grantee survey data show that about two-thirds of the projects admit new students during both the academic and summer sessions, suggesting that program openings are routinely filled (Fasciano and Jacobson, 1995).

Some reasons that students cited for leaving Upward Bound include their family moving away from the project or time conflicts, but the primary consideration for students is a job (see Table II.3). Looking at the reasons both for leaving Upward Bound and for not participating (Table II.1), it appears that employment was an important consideration for these generally low-income students. The finding on the employment/persistence relationship also coincides with reports from Upward Bound project directors who said that students typically drop out of Upward Bound to take a job during the summer or academic year (Fasciano and Jacobson 1995).

While employment appears to be important for Upward Bound participants, few participate in Job Training Partnership Act (JTPA) or work-study programs offered through Upward Bound projects. Data collected from project directors about students' Upward Bound activities suggest that only about four percent of the students participate in a work experience program associated with the Upward Bound project during the summer and less than one percent participate in one of these programs during the school year.[6] The relationship between employment and participation and/or persistence and the limited work-related experiences acquired by participants suggests that one strategy for retaining students may be to expand job opportunities through the program. In part, this is supported

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[6]In part, this reflects that many of the students are too young to work. Linking the grantee survey data and the student data shows that about 50 percent of Upward Bound participants during the 1993 summer session were enrolled in a project that offered some kind of work experience such as JTPA. Among those who participated in the 1993-94 academic year only 8 percent were enrolled in projects that offered some kind of work experience activity.

TABLE II.3

PERCENTAGE OF STUDENTS LEAVING UPWARD BOUND, BY REASON CITED

Reason	Percentage of Students[a]
Took a job	31
Family moved	14
Time conflict	11
Asked to leave the program	10
Grades low	9
Family issues	7
Pregnancy	6

[a]Percentages do not sum to 100 because students were allowed to provide multiple responses.

by the survey of Upward Bound project directors. Fasciano and Jacobson (1995) showed that program retention was higher in projects offering year-round work experience than in those with less than an all-year program.

In addition to the reasons for leaving Upward Bound, we looked at the role of family background characteristics in predicting persistence. As in the analysis of participation, we discovered that few student background factors have an independent effect on students' persistence. To assess the effect of student and family background factors on persistence, we used an analytic model that predicted the monthly rate of leaving the program.[7] From the model we can learn how the relative chances of leaving Upward Bound are influenced by family background factors. Two factors stand out as predictors of persistence: (1) students' race/ethnicity and (2) students' educational plans. Students

[7]Model estimates were derived from a proportional hazards model, and standard errors were computed taking into account the complex sample design.

planning to complete less than a college degree were more likely to leave Upward Bound than other students. We also found that Asian students were about one-third as likely to leave as African American participants (see Table II.4). Native American students left Upward Bound at about half the rate of African Americans. None of the other background factors appear to influence persistence. The project director's rating, which indicates how likely students were to be selected for Upward Bound using normal selection procedures, is also uncorrelated with persistence.

C. RANGE AND INTENSITY OF UPWARD BOUND SERVICES RECEIVED BY PARTICIPANTS

- The typical number of academic and nonacademic sessions attended each year was 274. Two-thirds of these sessions occurred during the summer and the rest took place during the academic year.
- The principal academic activities were instruction in math and English.
- Among nonacademic activities, most students participated in college preparation sessions, counseling sessions, and skill development activities. Counseling generally focused on personal, academic, career, and group-dynamic-related issues.

During the summer of 1994, we asked Upward Bound project directors to report about the academic and nonacademic services students had received since entering the program. Reports of participation show an intense program that includes both academic and nonacademic activities (see Tables II.5 and II.6).[8] Overall, we found students were most likely to attend English and math courses,

[8]Analyses reported in Tables III.5-III.7 are based on only those participants who had an opportunity to participate in at least one Upward Bound academic year program and one summer program. Students in this group were randomly selected for Upward Bound before August 1, 1993.

TABLE II.4

ADJUSTED RELATIVE RATES OF LEAVING UPWARD BOUND

Background Factor	Relative Rate of Leaving Upward Bound
Female[a]	.60
Hispanic[b]	.86
Asian[b]	.35**
White[b]	.78
Native American[b]	.52*
Other race[b]	.81
Grade level[c]	1.15
Low income/first generation[c]	1.09
Upward Bound project director's rating	1.01
Worked for pay during summer	1.19
Plans to complete at least a college degree	.73**

*Statistically significant at the .10 level.
**Statistically significant at the .05 level.

[a]Reference group is male.
[b]Reference group is African American.
[c]Coded as grade 8, 9, 10, 11, and 12.
[d]Reference group is low income only and first generation only.

TABLE II.5

STUDENT PARTICIPATION IN UPWARD BOUND ACADEMIC COURSES AND NONACADEMIC ACTIVITIES DURING SUMMER 1993

		Typical Number of Sessions Attended[a]	
	Percentage Enrolled	All Students[b]	Enrolled Students[b]
All courses:	99	100	100
English	87	27	30
ESL	0	0	24
Foreign languages	19	0	24
Math	96	24	24
Computers	23	0	27
Science	69	20	24
Social science	13	0	24
Elective	74	20	22
Other	8	0	24
All activities:	100	83	83
College preparation	92	13	19
Career exploration	43	0	3
Self-awareness	63	1	3
Field trips	90	4	5
Cultural awareness	51	1	2
Counseling	93	18	20
Skill development	85	24	25
Other	7	0	1

[a] "Typical" refers to the median number of sessions attended.
[b] "All students" refers to all Upward Bound participants regardless of whether they actually enrolled in a course. "Enrolled students" refers to only those who were actually enrolled.

TABLE II.6

STUDENT PARTICIPATION IN UPWARD BOUND ACADEMIC COURSES AND NONACADEMIC ACTIVITIES DURING THE 1993-94 ACADEMIC YEAR

		Typical Number of Sessions Attended[a]	
	Percentage Enrolled	All Students[b]	Enrolled Students[b]
All courses:	70	39	64
English	65	14	20
ESL	0	0	16
Foreign languages	4	0	16
Math	58	6	20
Computers	6	0	23
Science	40	0	20
Social science	6	0	14
Elective	40	0	20
Other	3	0	7
All activities:	100	51	51
College prep	90	8	10
Career exploration	18	0	1
Self-awareness	61	1	3
Field trips	71	2	3
Cultural awareness	57	1	2
Counseling	95	12	12
Skill development	89	16	20
Other	5	0	1

[a] "Typical" refers to the median number of sessions attended.

[b] "All students" refers to all Upward Bound participants regardless of whether they actually enrolled in a course. "Enrolled students" refers to only those who were actually enrolled.

and that the typical number of sessions attended during a year was 274.[9] About two-thirds of the sessions took place during the summer, and the remainder were taken during the academic year.

The principal academic activities in Upward Bound summer programs include instruction in English, math, and science. For example, almost all students in the summer program took at least one math course, more than three-quarters took an English course, and almost three-quarters took a science course. Fewer than one-quarter took a foreign language, computer science, or social science course. Generally, students who enrolled in a course attended about 24 sessions in each subject area during the summer. For English, enrolled students attended about 30 sessions.

Participation was also high in nonacademic activities. More than three-quarters of the students participated in college preparation, counseling, and skill development activities (that is, tutoring and development of study skills). The typical participant attended 19 to 25 sessions, and most students participated in at least one field trip. The most common topics covered in the counseling sessions included personal, academic, career, and group dynamic-related issues (see Table II.7). Family counseling and financial aid counseling were reported for only a few students; this may reflect the young age of the participants and may become more prevalent as they progress through high school. Looking across all service areas, we found that during the summer the typical participant attended about 100 academic sessions and 83 nonacademic sessions.

During the academic year between two-thirds and three-quarters of the students took an academic course and students attended fewer sessions than during the summer (about 39 sessions). Students most often took courses in English and math. There was no other subject area in which the majority

[9]"Typical" refers to the median number of sessions. For example, if the median number of sessions attended is 274, then this means 50 percent of the participants attended fewer than 274 sessions and 50 percent attended more than 274 sessions.

TABLE II.7

STUDENT PARTICIPATION IN UPWARD BOUND COUNSELING SERVICES

		Typical Number of Sessions Attended[a]	
	Percentage Enrolled	All Students[b]	Enrolled Students[b]
Summer 1993			
All counseling:	93	18	20
Personal	63	3	4
Family	13	0	3
Academic advising	56	1	3
Financial aid	19	0	5
Career	57	1	2
Group dynamics	62	5	10
Academic year 1993-94			
All counseling:	95	12	12
Personal	63	2	5
Family	16	0	2
Academic advising	63	2	3
Financial aid	22	0	2
Career	54	1	2
Group dynamics	54	1	14

[a]"Typical" refers to the median number of sessions attended.
[b]"All students" refers to all Upward Bound participants regardless of whether they actually enrolled in a course. "Enrolled students" refers to only those who were actually enrolled.

of students took a course. Within individual subject areas, enrolled students generally participated in between 15 and 20 sessions. As with the summer program, more than three-quarters of all students took part in college preparatory, counseling, and skills development activities. The typical number of sessions attended in these nonacademic areas was 10-20 sessions.

D. CONCLUSIONS

This chapter examined students' persistence in the Upward Bound program and the range of services students receive from the program. We found that one-fifth of the applicants offered slots chose not to participate. Some reasons given for not participating include employment-related considerations, transportation problems, and family issues. Only two student background characteristics were correlated with participation: (1) students' race/ethnic background and (2) grade level when applying for Upward Bound. We found that Hispanic and Asian students relative to African American students and younger students relative to older students were more likely to participate if offered a program slot.

Looking at program persistence, we learned that almost 40 percent of the participants will leave the program within 12 months. The most common reason students gave for leaving the program was to take a job. Background factors correlated with leaving the program include students' race/ethnic background and their educational plans before entering Upward Bound: Asian and Native American participants in contrast to African American participants and participants who expected to complete at least a four-year college degree were the most likely to persist.

III. SHORT-TERM IMPACTS OF UPWARD BOUND

Upward Bound's goals include increasing participants' skills and motivation so that they may succeed in education beyond high school. The program's ability to meet these goals can be assessed by examining many outcomes. Some outcomes that we explore include students' attitudes and educational expectations, course taking and grades, in-school behavior, and parents' involvement in education-related activities. Outcomes such as high school completion and college attendance and persistence must wait until the second follow-up report in late 1997 when students in the sample are old enough to have made these transitions.[1] We begin this chapter by presenting a context for interpreting the findings and the overall impacts for Upward Bound. Next, we describe the program impacts for all applicants and all participants and for specific subgroups of students.

A. A CONTEXT FOR INTERPRETING PROGRAM IMPACTS

We must consider several contextual factors when interpreting the impact of Upward Bound: (1) students' grade in school when they applied, (2) average exposure to the program, and (3) differences between Upward Bound applicants and members of the control group in the academic instruction and counseling they received from other programs or high schools.

- ***Most applicants were high school freshman or sophomores.*** Three-quarters of the students applying for Upward Bound in this evaluation had just completed grades 8 or 9. Moreover, the average time between application and the first follow-up data collection was about 12 months. This means the analyses conducted for this report capture students' high school experiences during their freshman or sophomore year.

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[1]Only 30 students in our sample dropped out of high school between the baseline survey and the end of the 1994 school year. Weighting the data to reflect the sample design, we find that about one percent of the control group dropped out of high school and about two percent of the treatment group dropped out of school. In large part this low dropout rate may reflect the young age of students who were recruited by the Upward Bound projects. Given the small number of dropouts at this point we chose not to analyze this outcome.

- ***Students had a limited amount of exposure to Upward Bound.*** We know from the findings presented in Chapter II that about 20 percent of those randomly selected for Upward Bound chose not to participate and almost 40 percent of the participants had left Upward Bound during the first 12 months. These facts taken together tell us that about 50 percent of the participants received no more than 8 months of exposure to Upward Bound services.

- ***Many students in the control group received some instruction or other services.*** More than 40 percent of the control group received some tutoring, counseling, or participated in academic workshops during the school year, and more than 10 percent of the control group received services during the summer. Anecdotal evidence from Upward Bound project directors suggests, for example, that some members of the control group participated in the federally funded Talent Search program. However, the services received by members of the control group are probably much less intense than those offered through Upward Bound. Also, while 40 percent of the control group received some limited set of services, 60 percent appear to have received no supplemental services.

B. THE AVERAGE IMPACT OF UPWARD BOUND

- Participants and parents of participants had higher educational expectations than members of the control group.

- Participants earn more academic credits during high school, particularly in English, social studies, and science. Impacts related to credits are in part a product of some high schools offering credits for completing courses offered through the Upward Bound program, and in part a product of students earning additional credits for completing high school courses.

- Participants who have been in Upward Bound for a longer period earn more credits in high school than other participants.

- Students most likely to benefit from Upward Bound include Hispanic students and students with low initial educational expectations.

- Other student-related outcomes such as grade point average, attitudes, parental involvement, and students' behavior in school were not affected by participation in Upward Bound.

The central question we address in this chapter is: On average, how do the educational outcomes of Upward Bound applicants and participants compare with what they would have achieved without

Upward Bound? In this section we explore this question from two perspectives.[2] First, using students who were randomly assigned to Upward Bound and a control group, we compare the average gain for each group to estimate the impact of Upward Bound.[3] Second, using only students selected for Upward Bound, we compare the outcomes of students who participated for different amounts of time. This second approach does not use random assignment for placing students into Upward Bound groups with different levels of exposure to the program; rather, students decide how long to remain in Upward Bound. As a consequence, estimates of program impacts may be biased because of self-selection; that is, participants who stay in the program for longer periods may differ systematically from those who dropout. For example, participants with longer duration in Upward Bound may be more motivated and may have had more positive high school experiences than other participants. Below, we present results derived from each of these approaches.

1. Program Impacts Derived from the Random Assignment Design

Our impact analysis looked across many student outcomes including course taking, grades, educational expectations, in-school behavior, attitudes, and parental involvement. When we examined the impacts within each of these areas we discovered that effects are concentrated in two key areas:

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[2]We also examined differences in impacts among projects to assess whether some projects were more effective than others. This analysis showed that for course taking and grades there is significant variation in project impacts, and that most projects have similar impacts on educational expectations. Further analysis looking at the relationship between project characteristics and variation in impacts revealed inconsistent results. Some project characteristics included in the analysis were urban location, size, type of host institution, predominant race/ethnicity, and curricular emphasis. Our final report on longer term project impacts will revisit this line of inquiry.

[3] We use the average gain for estimating program impacts instead of the average outcome at the time of the follow-up survey. Gain scores are used for two reasons. First, given the generally positive correlation between students' characteristics measured during the baseline and the follow-up surveys, program impacts are estimated with greater precision when using gain scores. Second, by using gain scores we remove potential differences between members of the treatment and control group that may have arisen by chance.

(1) course taking (academic preparation) and (2) changes in students' educational expectations (see Table III.1).[4,5] Results presented in the table show the average change in an outcome for students selected for Upward Bound and for members of the control group. We also show an adjusted estimate of program impact for participants. Most of the discussion focuses on the results for participants.

a. Credits Earned in High School

A typical high school student is expected to complete about five academic and/or elective courses each year. A closer look at state graduation requirements shows that the average number of credits in English, social studies, foreign languages, math, and science students must complete is about 3.3 credits each year (NCES Digest of Education Statistics, 1993).[6] When we look across all academic subject areas (math, science, English, foreign languages, and social studies), we find that Upward

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[4]Tests of statistical significance for program impacts are one-tailed tests with the probability of falsely rejecting the hypothesis of no impact set at .10. Using a one-tailed test means that we treat situations where there are no impacts and negative impacts the same. That is, our principal focus is on whether Upward Bound improves student outcomes.

[5]While Upward Bound has positive impacts for students' academic preparation and educational expectations, we also observe that participants are more likely to report being late for school and that their parents are less likely to attend a school event. Among other measures of school-related behavior, we generally find a similar pattern, that is, Upward Bound participants reporting higher levels of misbehavior. In these instances, however, the impacts are not significant. When we ask students how often their parents check or help with homework, talk to students about their grades or studies, and similar items we find that Upward Bound students often report less involvement, but again when compared with the control group the difference is not significant.

[6]Some states list both general and advanced requirements. When computing the average, we took the larger of the two.

TABLE III.1

IMPACT OF UPWARD BOUND ON CHANGES IN STUDENT OUTCOMES

Student Outcome	Gain for Upward Bound	Gain for Control Group	Impact (All Students)	Impact (Participants)[a]
Academic preparation:				
Grade point average	.03	.00	.03	.04
Credits earned in science[b]	1.12	.96	.15**	.18**
Credits earned in math[c]	1.34	1.21	.13**	.16**
Credits earned in English[d]	1.85	1.63	.22**	.26**
Credits earned in foreign languages[e]	.93	.82	.11*	.13*
Credits earned in social studies[f]	1.27	1.09	.18**	.22**
Total academic credits earned[g]	6.51	5.72	.79**	.95**
Credits earned in vocational education[h]	1.00	.79	.21*	.25*
Credits earned in remedial math[i]	.18	.15	.03*	.04*
Educational expectations and attitudes:				
Students'(years of schooling expect to complete)	-.34	-.53	.19*	.23*
Fathers'(years of schooling expect student to complete)	.05	-.22	.26**	.31**
Mothers'(years of schooling expect student to complete)	.09	-.15	.24*	.29*
Self-esteem[j]	.01	.02	-.01	-.01
Locus of control[k]	-.02	.00	-.02	-.02
How often talked with parents:[l]				
About selecting courses	.07	.12	-.05	-.06
About school activities	.00	.13	-.13	-.16
About studies	.10	.18	-.08	-.10
About grades	.05	.09	-.04	-.05
About transferring to another school	.05	-.04	.09*	.10*
About taking or preparing for theACT/SAT	.48	.55	-.07	-.08

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TABLE III.1 (*continued*)

Student Outcome	Gain for Upward Bound	Gain for Control Group	Impact (All Students)	Impact (Participants)[a]
About going to college	.28	.29	-.01	-.01
How often parents:[m]				
Checked on homework	.04	-.08	.12	.14
Helped with homework	.00	-.18	.18	.22
Gave special privileges for good grades	.13	.14	-.01	-.01
Limited privileges because of poor grades	-.18	-.12	-.06	-.07
Required chores around the house	-.08	-.03	-.05	-.06
Limited time watching TV or playing video games	-.16	-.12	-.04	-.05
Limited time with friends	-.17	-.07	-.10	-.12
Attended a school meeting	-.04	-.07	.04	.05
Spoke with teachers	.01	.01	.00	.00
Visited classes	-.08	.00	-.08	-.10
Attended a school event	-.07	.02	-.09	-.11
Misbehavior:[n]				
Times late for school	.92	.44	.48**	.58**
Times skipped classes	.42	.38	.04	.05
Times missed a day of school	.76	.61	.15	.17
Times in trouble for not following school rules	-.09	-.22	.13**	.16**
Times put on in-school suspension	.00	.00	.00	.00
Times suspended	-.01	-.01	.00	.00
Times arrested	.04	.04	.00	.00
Times spent time in juvenile home	-.01	.00	-.01	-.01

*Indicates statistical significance at the .10 level using a one-tailed test.
**Indicates statistical significance at the .05 level using a one-tailed test.

[a]Estimates for participants reflect using an adjustment for some students not showing up for Upward Bound after being offered an opportunity to participate. Estimates for participants are computed as impact (participants) = impact (all students)/(proportion of students showing up for services).

TABLE III.1 (*continued*)

[b]Science includes courses in earth science, biology, chemistry, and physics.

[c]Math courses include Algebra 1, Geometry, Algebra 2, Trigonometry, Pre-Calculus, and Calculus.

[d]English includes literature, grammar, communications, and English as a Second Language courses.

[e]Foreign languages include courses in Spanish, French, Chinese, Latin, and Sign Language.

[f]Social studies includes courses in American politics, history, sociology, psychology, and economics.

[g]Total academic credits includes credits in math, science, English, social studies, and foreign languages.

[h]Vocational education includes both non-occupationally specific and occupationally specific courses. Non-occupationally specific courses include, for example, business math, cosmetology, and home economics. Occupationally specific courses include agricultural business and management, soil sciences, accounting, wild life management, and hotel and motel management.

[i]Remedial math courses include courses in both basic and general math.

[j]Self-esteem is based on responses to seven statements. The statements include "I feel good about myself," "I feel I am a person of worth, the equal of other people," "I am able to do things as well as most other people," "On the whole, I am satisfied with myself," "I certainly feel useless at times," "At times I am no good at all," and "I feel I don't have much to be proud of." Students were asked to indicate if they strongly agree, agree, disagree, or strongly disagree with each statement. Responses to the statements were transformed into standard deviations and summed to form a composite score for each student.

[k]Locus of control is based on student responses concerning six statements. The statements include "I don't have enough control over the direction my life is taking," "Every time I try to get ahead, something or somebody stops me," "In my life, good luck is more important than hard work for success," "My plans hardly ever work out, so planning only makes me unhappy," "When I make plans I am almost certain I can make them work out," and "Chance and luck are very important for what happens in my life." Students were asked to indicate if they strongly agree, agree, disagree, or strongly disagree with each statement. Responses to the statements were transformed into standard deviations and summed to form a composite score for each student.

[l]Coded as never, rarely, sometimes, and often.

[m]Coded as never, rarely, sometimes, and often.

[n]Coded as midpoints for the following categories: 1-2 times, 3-6 times, 7-9 times, 10 or more times.

Bound increases participants' academic course taking by an additional credit during a period of one to two years.[7] This means that on average, Upward Bound participants earned one more credit in an academic subject (or completed one more course) than similar students who did not participate in Upward Bound.[8]

When we look more closely at the subjects that contribute to these gains, we find that Upward Bound participants earn substantially more credits than members of the control group. The average gain in the number of credits for some subjects is large. Upward Bound participants, for example, earn almost .3 more credits in English than those in the control group. We find similar, but smaller impacts for social studies, science, math, and foreign languages. Besides increasing the number of academic credits earned, we observe an increase in the number of credits earned in vocational courses and remedial math; however, the impact on remedial math course work is not substantial. We find no impact on participants' grade point average.

To appreciate the size of these impacts on participants' course taking, we can imagine the educational experiences of students where we provide Upward Bound services to some and compare these with other students who are the same in all ways except one: they did not participate in Upward

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[7]Analyses of changes in course taking are based on Upward Bound applicants who were in grades 10-12 when they completed the baseline questionnaire. Using this sample we can construct for each student the number of credits earned before Upward Bound could have affected their course taking behavior. This serves as the baseline value when computing change scores. The number of students for which we estimate impacts on credits is about 1,200 students. Almost 650 of these students are in the treatment group and the remainder are in the control group. On average, the time between the end of 9th grade and the end of the 1993-94 school year was 1.7 years. Older (students in grades 10 and 11) and younger Upward Bound applicants (students in grade 9 or younger when applying for Upward Bound) had similar educational expectations before entering Upward Bound. However, fewer of the older applicants were black and there was a somewhat larger concentration of students in rural projects among the older applicants.

[8]Credits earned by students are defined in terms of Carnegie units. A Carnegie unit corresponds to a course that meets for 45-60 minutes, five days a week, for an entire academic year. Credits earned equals credits attempted only when a student successfully completes a course with a passing grade.

Bound. We expect that under these conditions, *members of the control group* would on average, during their sophomore or junior year of high school, earn 1.63 English credits. *Upward Bound applicants* on the other hand, will earn more than 1.85 credits in English courses during this same period. This difference amounts to an impact of about .2 credits or almost .3 credits for participants. An impact of .3 credits means that Upward Bound participants are exposed to about 17 percent more instruction in English than other, similar students. Overall, we reach a similar conclusion; that is, Upward Bound participants are exposed to 17 percent more academic instruction than other students.

Upward Bound's impact on academic course taking can be attributed to two related processes: (1) having students take and complete at least one academic course during the school year and (2) increasing students' academic course load.[9] Table III.2 shows the results of looking at the impact of Upward Bound on shifting students into an academic curriculum. This table shows that the program increases the percentage of students participating in academic course work by four points. Using these results we can estimate that about one-quarter of the impact on total academic credits earned can be attributed to shifting some students into an academic curriculum.[10] Most of Upward Bound's impact, however, is a result of participants already in an academic curriculum taking more academic courses.

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[9]We categorize students as participating in an academic track if they earn some credit in at least one academic subject area (math, science, English, social studies, or foreign languages--see footnotes for Table III.1, for a list of courses included under each subject area). Students who attempted an academic course but did not achieve a passing grade are not counted as part of this group. About five percent of the sample did not earn any academic credits between the end of the 9th grade and the end of the 1993-94 school year. A more detailed examination also shows that almost 13 percent of these students dropped out of school after applying for Upward Bound and completed few credits in subjects such as vocational education and remedial math. In contrast, those earning at least one credit were more likely to remain in school, earn credits in vocational education and remedial math, and have high educational expectations. We discuss the decomposition of the impact estimates in the technical appendix.

[10]We describe an approach for drawing this conclusion in Technical Appendix D.

TABLE III.2

PERCENT OF STUDENTS EARNING AT LEAST ONE ADDITIONAL ACADEMIC CREDIT, BY TREATMENT STATUS

	Upward Bound	Control Group	Impact (All Students)	Impact (Participants)
Percent participating in academic track	96%	93%	3%**	4%

**Significant at the .05 level.

b. High School Credits Earned for Upward Bound Courses

Upward Bound project directors often report that some or all of their participants are given high school credit for completing Upward Bound courses (Fasciano and Jacobson 1995).[11] This finding suggests that much of the program's impact on course taking may be a direct product of completing Upward Bound courses and not an outcome of better performance in the high school classroom. To explore this relationship, we look at the differences in impacts by the percentage of students receiving high school credits for Upward Bound courses, as reported by the project directors. The results presented in Table III.3 are mixed. Several conclusions, however, can be drawn from this table. First, even in projects where no high school credits are offered we find positive impacts. Second, about 40

[11]Among Upward Bound projects, 54 percent report none of their students receives high school credits for completing Upward Bound courses; 27 percent say some participants receive credit; and 19 percent of projects report all of their students receive credits for Upward Bound courses. Comparison with RTI's previous evaluation shows that the percentage of projects offering high school credits has remained fairly constant since the 1970s.

TABLE III.3

RELATIONSHIP BETWEEN HIGH SCHOOLS OFFERING CREDITS FOR UPWARD BOUND COURSES AND THE IMPACT OF UPWARD BOUND ON HIGH SCHOOL CREDITS

Outcome	No Credits Offered to Participants	Less than Half of the Participants Receive Credit	More than Half but Not All of the Participants Receive Credit	All Participants Receive Credit	Test of No Interaction[a]
Grade point average	-0.03	0.03	-0.02	0.43**	*
Credits earned:					
Science	0.17**	-0.02	0.06	0.31**	**
Math	0.16**	0.03	0.23**	0.26**	
English	0.14**	0.10	0.43**	0.73**	*
Foreign languages	0.17*	-0.05	0.06	0.17**	
Social studies	0.17**	-0.04	0.02	0.62**	*
Total academic credits	0.80**	0.01	0.78**	2.07**	**
Vocational education	0.28**	0.28**	-0.13	-0.09	**
Remedial math	0.02*	-0.09	-0.07	0.21**	**

*Significant at the .10 level.
**Significant at the .05 level.

[a]Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

percent of the overall impact can be attributed to projects where high schools offer credit for Upward Bound courses.[12] Taking these two facts together suggests that while high school credits may be awarded for taking Upward Bound courses, substantial effects remain for projects not offering high school credits. The lack of impact for projects where fewer than all participants receive high school credits is contrary to the pattern we would generally expect to find. At this point we have not developed an explanation for this inconsistency.

While we cannot directly address the issue of whether Upward Bound courses are of the same quality as courses offered by high schools, we would expect that the Upward Bound courses must meet at least some minimum standards for content and classroom hours before school districts will grant credits for these courses. Indirect evidence of this practice is shown in Table III-4 where we examine subject areas for which we observed substantial differences in program impacts in Table III-3. Here, we see that students in projects offering high school credit generally attend more Upward Bound class sessions than participants in other projects.

c. Educational Expectations

Besides finding program impacts on academic preparation, we discovered that participants and their parents had substantially higher educational expectations than members of the control group (see Table III-1). During the baseline and follow-up surveys, we asked students to describe how far they thought they would go in school, given their current situation. In addition, we asked the students to tell us how far they thought their father and mother expect them to go in school. We find that the

[12]The computation is based on the percentage of students falling within each of the project types (1) 63 percent, (2) 17 percent, (3) 5 percent, and (4) 15 percent, respectively.

TABLE III.4

AVERAGE NUMBER OF SUMMER CLASS SESSIONS ATTENDED, BY ACADEMIC SUBJECT AREA

Subject Area	No Credits Offered to Participants	Less than Half of the Participants Received Credit	More than Half But Not All of the Participants Receive Credit	All Students Receive Credit
Writing/composition	22	25	26	30
Literature	24	28	32	21
Reading comprehension	21	25	21	33
Biology[a]	20	29	22	31
History[b]	23	11	13	32
Math	22	27	27	31

[a]Other science subject areas had fewer than 30 students attend a course.

[b]Other social studies subject areas had fewer than 30 students attend a course.

difference in gains in expectations for Upward Bound participants and members of the control group is .2 years.[13] We also find an increase in fathers' and mothers' educational expectations.

2. Duration in Upward Bound and Program Effects

Table III.5 shows the average change in students' outcomes by the length of exposure to Upward Bound. Most gains in student outcomes occur within the area of academic preparation. We find that

[13]Looking more closely at students' educational expectations shows that expectations for both Upward Bound participants and members of the control group generally declined as students moved through school, but the expectations of Upward Bound participants declined less. We checked these experiences against those of students in the National Educational Longitudinal Study. In doing so, we found a similar pattern--declining expectations--among other students who had high expectations in 8th grade. Students with low expectations generally experienced increased expectations as they moved, for example, from grade 8 to grade 10.

TABLE III.5

GAINS IN STUDENT OUTCOMES, BY MONTHS IN UPWARD BOUND

Student Outcome	1-6 Months	7-12 Months	12+ Months
Academic preparation:			
Grade point average	.04	.01	-.04
Credits earned in science	.96**	.99**	1.35
Credits earned in math	1.18**	1.14**	1.76
Credits earned in English	1.61**	1.70**	2.16
Credits earned in foreign languages	.64**	.97	1.10
Credits earned in social studies	1.12*	1.09**	1.52
Total academic credits earned	5.52**	5.90**	7.89
Credits earned in vocational education	.62**	.66	1.85
Credits earned in remedial math	.21	.16	.16
Educational expectations and attitudes:			
Students'	-.63	-.71	-.07
Fathers'	.19	-.26**	.36
Mothers'	-.13	-.08	.13
Self-esteem	-.09*	-.01**	.08
Locus of control	-.14*	.00	.04
How often talked with parents:			
About courses	.04	.13	.02
About school activities	-.12	.13	.01
About studies	-.22	.22	.15
About grades	-.21	.13	.08
About transferring to another school	.06	.16**	-.14
About taking or preparing for the ACT/SAT	.29	.48	.54
About college	.06	.44	.26

TABLE III.5 (*continued*)

Student Outcome	1-6 Months	7-12 Months	12+ Months
How often parents:			
Checked on homework	.21	.01	-.01
Helped with homework	-.18	.22**	-.05
Gave special privileges for good grades	.04	.19	.18
Limited privileges because of poor grades	-.29**	-.24	.07
Required chores around the house	-.18	.03**	-.11
Limited time watching TV or playing video games	-.34	-.15	-.11
Limited time with friends	-.20	-.03	-.26
Attended a school meeting	-.07	-.10	.03
Spoke with teachers	.05	-.01	.02
Visited classes	-.07	-.06	-.14
Attended a school event	-.17	-.03	-.10
Misbehavior:			
Times late for school	1.16	.74	.93
Times skipped classes	.95	.53	.16
Times missed a day of school	.78	.69	.54
Times in trouble for not following school rules	-.02	-.06	-.22
Times put on in-school suspension	-.16	-.04	-.06
Times suspended	.01	.04	-.06
Times transferred for disciplinary reasons	.00	.00	.00
Times arrested	.03	.06	.05
Times spent time in juvenile home	.12	.02	-.03

*Statistically significant at the .10 level when compared to students in the program for more than 12 months.

**Statistically significant at the .05 level when compared to students in the program for more than 12 months.

youth who participated for more than 12 months completed courses in more academic subjects than students with less exposure to Upward Bound. For example, those who were in the program for more than 12 months earned about 1.4 science credits while participants with less than 12 months exposure earned only one credit. Overall, participants with 12 or more months of exposure acquired almost 8 additional academic credits between the end of grade 9 and the time of the follow-up data collection, and participants with 7-12 months of Upward Bound service earned an additional 6 credits. Those with 1-6 months exposure also earned almost 6 additional credits.

C. WHO BENEFITS MOST FROM UPWARD BOUND?

In this section we assess whether specific groups of students are more likely to benefit from Upward Bound than others. Unlike the analysis looking at exposure to Upward Bound, the subgroup analysis relies on a comparison of the treatment and control groups; thus, we return to a discussion of program impacts measured by differences in average gains. We find differential impacts by race/ethnic background and by initial level of educational expectations.[14] We find few differences by sex, by low income/first generation status, or by prior academic performance.[15] When we compare students by project director's rating of the likelihood of being selected under normal selection rules or by selection date for Upward Bound,[16] we also find no systematic differences. Results from

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[14]After exploring the relationship between participation in Upward Bound and its impact for specific groups of students defined by race/ethnicity and educational expectations, we examined whether the combination of race/ethnicity and educational expectations produced different levels of impacts. These analyses suggested that differences between racial/ethnic groups by level of expectations were similar. They also showed that differences in impacts for participants with high and low expectations by racial/ethnic group were similar.

[15]Some comparisons suggest that there may be differential impacts on parental involvement for these groups; however, the evidence to support this contention is quite limited.

[16]Selection date for Upward Bound indicates the date on which projects selected students to participate in Upward Bound. Students randomly selected for Upward Bound before August 1, 1993 could have participated in the program for a year or more before being administered the first follow-up

comparisons based on these last five variables are shown in Technical Appendix F. The impacts by race/ethnicity and expectations are described below.

1. Impact of Upward Bound by Race/Ethnic Background

This analysis looks at the three largest racial/ethnic groups of students in Upward Bound: (1) African Americans, (2) whites, and (3) Hispanics.[17] As with prior analyses, most differences in program impacts are found among outcomes related to academic preparation (see Table III.6).[18] Here a consistent picture emerges among the three groups: we find that Hispanic students routinely experience larger returns from participating in Upward Bound than either African American or white participants. In particular, we find that the impact of Upward Bound on the total number of academic credits earned is much larger for Hispanic participants. For Hispanic students we find an increase of more than 2 credits, and for African American and white students Upward Bound has an impact of less than .5 credits. Differential returns of participating in Upward Bound for Hispanics can be seen in several subject areas including: math, English, vocational education, foreign languages, and social studies.

To better understand how the differences among the three groups are generated, we examined two related elements of students' course taking while in high school: (1) participation in an academic

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survey. That is, this group of students had an opportunity to participate in at least one academic year and summer program. Other students had less of an opportunity to receive a full range of Upward Bound services. The results in the appendix show no differences in impacts for the two groups; however, on key outcomes such as total credits earned we find that those with potentially greater exposure had numerically larger impacts.

[17]Overall, the three population groups make up about 94 percent of the students selected for Upward Bound as part of the evaluation. Native Americans and Asians comprise only a small number of students in the sample and as a consequence impacts estimates have little precision.

[18]To simplify the presentation we present only those results from the analyses most relevant to the discussion in the text. In Technical Appendix G we show impacts for all student outcomes.

TABLE III.6

UPWARD BOUND IMPACTS, BY RACE/ETHNICITY

Outcome	All Students			Test of No Interaction[1]	Participants		
	Black	White	Hispanic		Black	White	Hispanic
Academic preparation:							
Grade point average	0.02	0.06	0.02		0.03	0.07	0.02
Credits earned in science	(0.01)	0.10 *	0.34 **		(0.01)	0.12 *	0.38 **
Credits earned in math	0.04	0.06	0.36 **	*	0.05	0.07	0.40 **
Credits earned in English	0.10 *	0.16	0.44 **	**	0.13 *	0.19	0.49 **
Credits earned in foreign languages	(0.02)	0.04	0.47 **	**	(0.03)	0.05	0.52 **
Credits earned in social studies	0.08	0.02	0.37 **	**	0.10	0.02	0.41 **
Total academic credits earned	0.19	0.37	1.99 **	**	0.25	0.44	2.20 **
Credits earned in vocational education	0.02	(0.10)	0.70 **	**	0.03	(0.12)	0.77 **
Credits earned in remedial math	0.05	0.01	0.01		0.07	0.01	0.01

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

curriculum and (2) the number of academic credits earned by students participating in an academic curriculum.[19] By exploring these two dimensions of course taking we are able to assess whether Upward Bound shifts participants into an academic trajectory or if its participants engage in more academic course work.

We find that Upward Bound increases the percentage of Hispanic students in an academic program by about 13 percentage points (see Table III.7). Moreover, we find that almost all African American and white students would have participated in an academic curriculum regardless of Upward Bound. In the program's absence only 87 percent of the Hispanic students would have participated.

TABLE III.7

PERCENT OF STUDENTS EARNING AT LEAST ONE ADDITIONAL ACADEMIC CREDIT, BY RACE/ETHNICITY AND TREATMENT STATUS

Population	Upward Bound Participants	Control Group	Impact (Difference in Percentages)
Hispanic	99%	87%	13**
White	99	98	1
African American	92	96	-4

**Statistically significant at the .05 level.

Using the percentages displayed in Table III.7, we can simulate what the impact for Hispanics would have been if they had participation rates similar to African American or white youth (see Table III.8). Comparing the simulated estimates with the impacts for African American and white youth

[19]We categorize students as participating in an academic curriculum if they earn some credit in at least one academic subject area between 9th grade and the time of the transcript data collection. For most students this refers to a period of one year. Students who have attempted an academic course, but who did not achieve a passing grade are not counted as part of this group. We discuss the decomposition of the impact estimates in Technical Appendix E.

TABLE III 8

IMPACTS OF UPWARD BOUND ON ACADEMIC COURSE TAKING FOR HISPANIC STUDENTS[a]

Actual Impact on Hispanic Participants	Impact for Hispanics Using Rates for White Students	Impact for Hispanics Using Rates for African American Students
2.18	1.50	1.09

[a]All estimates adjusted to reflect no-shows.

shows the extent to which the gap in impacts is a product of differential participation in an academic curriculum. Making this comparison shows that the difference between Hispanic and white participants is reduced by about 40 percent[20] if Hispanics participate in an academic curriculum at the same rate as whites. For the Hispanic and African American comparison, we find that more than half of the gap is a function of differences in participation rates in academic courses.[21] These findings show that at least 40 percent of the differential impact for these three population groups comes about because Upward Bound is more likely to shift Hispanic students into an academic curriculum than other students. The remaining differences are a product of different course loads.

2. Impact of Upward Bound by Level of Educational Expectations

In this section we compare Upward Bound's impact for two groups of students: (1) those who, prior to participating in Upward Bound, expected to complete at least a four-year college degree and

[20] [1 - (1.50 - .44)/(2.18 - .44)].

[21] [1 - (1.09 - .25)/(2.18 - .25)].

(2) those with lower expectations.[22] Differential impacts are spread across many of the student outcomes and a fairly consistent pattern emerges: Upward Bound has larger short-term impacts for students with low expectations. For example, we find that Upward Bound increases parents' educational expectations to a greater degree if students started Upward Bound with lower expectations. Upward Bound increased fathers' educational expectations by 1.2 years for participants with low initial expectations and showed no increase for those with higher expectations (see Table III.9). A similar result occurs for mothers' expectations as well. We find no difference for changes in students' expectations.

Among the outcomes used to measure academic preparation, we find that for math, English, and social studies Upward Bound has a larger impact for students with low expectations. Upward Bound students with low expectations gained almost .6 more academic math credits than members of the control group while the corresponding figure for those with higher expectation is .1 credits. For both English and social studies we also find large impacts for students with low educational expectations. In both cases participation in Upward Bound appears to increase engagement in these subject areas by about .8 credits. Across all the academic subjects, we find that Upward Bound increased the number of academic credits earned by 3.1 for participants with low expectations and by .5 credits for those with higher expectations.

Using the same approach we used when comparing total academic preparation for different race/ethnic groups, we can show that much of the difference in impacts for students with low and high expectations occurs because Upward Bound shifts students with low expectations into an academic track (see Table III.10). Comparing the impacts for students with low and high expectations reveals

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[22]About three-quarters of the students selected for Upward Bound expected to complete at least a four-year college degree.

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TABLE III.9

UPWARD BOUND IMPACTS, BY EDUCATIONAL EXPECTATIONS

Outcome	All Students			Participants	
	Low Expectations	High Expectations	Test of No Interaction[1]	Low Expectations	High Expectations
Academic preparation:					
Grade point average	0.09	0.02		0.12	0.02
Credits earned in science	0.36 **	0.12 **		0.46 **	0.15 **
Credits earned in math	0.46 **	0.10 **	**	0.59 **	0.12 **
Credits earned in English	0.66 **	0.06	**	0.84 **	0.07
Credits earned in foreign languages	0.32 **	0.09 *		0.41 **	0.11 *
Credits earned in social studies	0.62 **	0.06	**	0.79 **	0.07
Total academic credits earned	2.43 **	0.41 **	**	3.11 **	0.50 **
Credits earned in vocational educatio	0.18	0.18 *		0.23	0.22 *
Credits earned in remedial math	0.15 **	(0.01)	**	0.19 **	(0.01)
Educational expectations and attitudes:					
Students'	0.15	0.06		0.19	0.07
Fathers'	0.94 **	0.01	**	1.20 **	0.01
Mothers'	0.71 **	0.09	**	0.91 **	0.11
Self-esteem	0.03	0.00		0.04	0.00
Locus of control	(0.01)	(0.01)		(0.01)	(0.01)

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

TABLE III.10

PERCENT OF STUDENTS EARNING AT LEAST ONE ACADEMIC CREDIT, BY COLLEGE EXPECTATIONS AND TREATMENT STATUS

Population	Percent of Treatments in Academic Program	Percent of Controls in Academic Program	Impact
No college degree	99%	88%	11**
Complete college degree	96	94	2

**Statistically significant at the .05 level.

that 42 percent of the impact differential can be attributed to differences in participation in an academic program.[23] This means that if Upward Bound students and members of the control group with low expectations participated in an academic program at the same rate as youth with higher expectations, we would compute an impact of about 2.0 credits instead of one of 3.1 credits (see Table III.11).

TABLE III.11

IMPACTS OF UPWARD BOUND FOR STUDENTS WITH LOW EDUCATIONAL EXPECTATIONS[a]

Actual Impact for Students with Low Expectations	Simulated Impact Using Academic Participation Rates for Students Expecting to Complete a College Degree
3.1	2.0

[a]Adjusted to reflect no-shows.

[23][1 - (2.0 - .5)/(3.1 - .5)].

D. CONCLUSIONS

Upward Bound's goals include increasing students' skills and motivation so that they can succeed in education beyond high school. With the data we have in hand we cannot look at the long-term impacts of Upward Bound and measure whether it was successful in meeting these goals; however, we can assess its short-term impacts on more intermediate outcomes. Upward Bound's short-term impacts are generally concentrated in two areas: (1) maintaining or increasing educational expectations of participants and their parents, and (2) increasing students' academic preparation through participants taking additional academic courses in subjects such as science, English, and social studies. Students most likely to benefit in the short term from participating in Upward Bound included Hispanic youth and those with low educational expectations before participating in Upward Bound.[24]

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[24]Findings about some groups of students benefitting from Upward Bound more than others must be interpreted with caution. A temptation when finding, for example, that students with low expectations benefit more than students with high expectations is to target the program on students with low expectations. Doing so, however, will change the student composition and, as a consequence, may change the project such that it will be less effective. For example, students with low expectations may be more motivated to succeed when they interact with students with high expectations. Focusing all resources on students with low expectations would remove this element of the program and diminish the benefits received by these students. Preliminary analysis of the relationship between variability in the mix of students in projects and project impacts shows, for example, that projects with greater variability in student expectations have larger impacts than those with less variability.

IV. CONCLUSIONS

The Upward Bound program is designed to improve the academic preparation of economically disadvantaged high school students so that they may succeed in postsecondary education. Drawing on data from Upward Bound project directors, students, and high schools transcripts, we found that Upward Bound projects generally offer intense summer and academic year programs that emphasize course taking in English, science, and mathematics, and provide counseling and tutoring services. The typical Upward Bound applicant is a rising 9th-grader or a high school freshman or sophomore. Girls are more likely to apply for Upward Bound than boys, and about three-quarters of the applicants expect to complete at least a four-year college degree.

A. SUMMARY OF MAJOR FINDINGS ON SHORT-TERM IMPACTS AND PERSISTENCE IN UPWARD BOUND

Examining data pertaining to the first year or two that students participate in Upward Bound leads to four major findings (see Table IV.1). First, many Upward Bound participants (40 percent) leave the program during the first year, particularly students with low initial educational expectations. We also found that African American students were more likely to leave than Asian and Native American students, and that many dropouts left the program for job-related reasons. Second, after examining a broad range of student outcomes, it appears that Upward Bound's short-term impacts are concentrated in two important areas: (1) changes in participants' course taking and (2) maintaining or improving participants' educational expectations. While both Upward Bound participants and nonparticipants take course loads that at a minimum parallel what might be expected for meeting typical state graduation requirements, Upward Bound participants earn more high school credits in English, social studies, foreign languages, math, and science. This suggests that at least for the short-

TABLE IV.1

SUMMARY OF FINDINGS

Population	Share of the Population	Persistence in Upward Bound Program	Earning Credits in Academic Courses	Changes in Educational Expectations
All students	100%	• Average duration: 8 months • 40 percent leave in first 12 months	• Upward Bound participants earn almost one more academic credit than nonparticipants • Participants spending 12 or more months in Upward Bound earn more academic credits than those with less exposure	• Participants expected to attend .25 more years of school than nonparticipants • Parents of participants expected their children to complete more school than their counterparts
Race/ethnicity[a]		• African American students are more likely to leave program than Asian or Native American students		
Hispanic	22%		• Hispanic participants completed two more academic courses than nonparticipants	• No difference in impacts among groups
White	21%		• No impact	• No difference in impacts among groups
Black	50%		• No impact	• No difference in impacts among groups
Educational expectations				
Low versus high	(23% and 77%)	• Students with low expectations have less exposure to Upward Bound because they leave sooner	• Impact on credits is greater for students with low expectations	• Impact on parents' expectations is greater for participants with low expectations

[a]Does not sum to 100 percent. Only the three largest race/ethnic groups in Upward Bound were analyzed separately.

term, Upward Bound is accelerating students' educational progress. We also found that Upward Bound participants generally maintain higher educational expectations than nonparticipants. Third, the short-term benefits of participation are most pronounced for two relatively small groups of Upward Bound students: Hispanics and those with low educational expectations.[1] Both groups comprise less than one-fourth of all new participants: about 21 percent of new participants are of Hispanic origin and about 23 percent of new participants expect to complete less than a college degree. We found no impacts or small impacts for non-Hispanic students and students with high expectations. Fourth, the early findings suggest that student outcomes are improved if participants remain in the program for a longer period. For example, we found that participants who were in the program for at least 12 months completed about 25 percent more academic course work than those who were in the program for less than 12 months.

B. HOW DO THESE FINDINGS COMPARE WITH PREVIOUS FINDINGS?

The last major evaluation of Upward Bound focused on a group of students who entered the program more than 20 years ago (Burkheimer et al. 1979). Findings from the earlier evaluation describe a program that did not influence participants' high school course taking and grades, increased their educational expectations and improved their access to postsecondary schooling, and had no effect on their postsecondary persistence. Students' persistence in Upward Bound itself was not assessed in the earlier evaluation, and the present evaluation cannot yet assess the program's impact on college enrollment.[2] However, where the two evaluations do overlap an important difference stands out: the

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[1]When comparing impacts for whites, African Americans, and Hispanics, we find differential impacts for course taking. When comparing students with high and low initial expectations we find differential impacts for course taking and educational expectations.

[2]In 1982, GAO reported findings on persistence that showed that, for the 12 projects visited, 50 percent of the students who entered Upward Bound subsequently dropped out of the program before graduating high school (GAO 1983).

current evaluation shows a program that has large impacts on students' high school course taking in academic subjects. A finding that is consistent across the two evaluations concerns educational expectations: participants have higher expectations than nonparticipants.

C. WHAT DO THE INITIAL FINDINGS TELL US ABOUT PROGRAM IMPROVEMENT?

The findings we report here focus on short-term impacts and do not tell us whether Upward Bound participants complete the same number of courses or more courses after attending four years of high school, nor do they tell us if Upward Bound increases the chances of attending and completing college--two important goals for the program. However, our initial look does suggest that larger impacts may be possible if Upward Bound projects were better able to hold students in the program, particularly students with low initial expectations. These participants are more likely to benefit from Upward Bound, yet they are also more likely to drop out of the program and less likely to apply. One approach for retaining these students is to place more direct emphasis on raising expectations of lower-aspiration students so that they see the possibilities available to them if they remain in school for a longer period. Another mechanism for retaining participants may be the provision of employment opportunities during the summer and school year. Options for adding an employment-related component to Upward Bound include extending the summer program by one or more weeks and spreading instruction over a longer period so that students may work a few hours each day during the Upward Bound summer program and after the summer session has ended. To sustain their jobs at least on a part-time basis during the school year, projects may need to curtail some activities so that students may work after school and for a few hours on weekends--a time that many Upward Bound projects use for tutoring and counseling. Other options may include, for example, integrating academic instruction into a work-related environment. With any of these approaches, introducing

employment into the mix of services will require careful balancing of program, school, and job-related demands for students' time.

D. ISSUES TO BE CONSIDERED IN THE NEXT REPORT ON PROGRAM IMPACTS

This report describes findings that may be a preview to those found after following these same students for four years rather than two years. Some outcomes that we will continue to explore in the next report on the program's impacts include persistence in Upward Bound, course taking during high school, and changes in expectations and school-related behavior. New topics will include engagement in activities related to college application (for example, test preparation and financial aid counseling), receipt of financial aid, type of postsecondary school attended, and persistence in college. Besides examining these outcomes, we will continue to investigate the relationships between project characteristics and program impacts to further our understanding of potential areas for program improvement. Some project characteristics we will consider are size, group composition, participant-to-staff ratio, and curricular emphasis. As a part of this work, we will also present a cost-effectiveness analysis of selected options.

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TECHNICAL APPENDIX A

SAMPLE DESIGN, WEIGHTING, AND ERROR ESTIMATION

In this appendix, we describe how the impact study sample was selected, how the baseline and first follow-up data were weighted, and how standard errors of sample estimates were calculated to reflect the sample design.

A. SAMPLE DESIGN

For the impact study, we selected a nationally representative sample of eligible Upward Bound applicants in two stages. First, we selected a nationally representative sample of Upward Bound projects to serve as "primary sampling units" (PSUs). Second, we selected eligible applicants to those projects and randomly assigned students to treatment and control groups.

1. First Stage Sampling: Selection of Projects

The "universe" of projects for the impact study--the collection of projects whose students are targeted for study and eligible to be selected for the study sample--consists of active regular projects that are (1) located in the 50 states and the District of Columbia; (2) hosted by postsecondary educational institutions; (3) mature, having operated for at least three years by October 1992; and (4) not serving only students with physical disabilities. Veterans projects and math/science projects are not considered regular projects. During the period when the impact study sample of students was being selected (roughly May 1992 through March 1994), there were 395 Upward Bound projects that met the definition of the universe.[1]

From the universe of 395 projects, we selected a sample of 70 projects using stratified simple random sampling: each project in the universe was assigned to a group of projects (a stratum) and a sample was drawn from each stratum. Sampling rates varied across strata, so some projects had a

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[1]Some projects funded in the 1989-92 grant cycle were defunded in the 1992-95 grant cycle and therefore eliminated from the universe. Projects newly funded in the 1992-95 grant cycle were also excluded from the universe.

greater chance of being selected than other projects. Stratification with disproportionate sampling (unequal sampling rates) was used to ensure that enough projects and, therefore, enough students were selected to support precise estimates for relatively small, but important analytic subgroups, such as students in large projects or students in projects hosted by two-year postsecondary institutions.

Table A.1 displays the 46 strata used to select projects in the first-stage sampling for the impact study. The table also shows, for each stratum, the number of projects in the universe, the number of projects selected for the sample, and the number of projects in which random assignment of students was carried out. Within each stratum, projects were selected using simple random sampling without replacement. Thus, although selection probabilities varied across strata, each project in a given stratum had the same chance of being selected. That chance equals the number of projects selected divided by the number of projects in the universe in that stratum.[2]

Strata are defined, in part, by cross-tabulating three stratifying variables:

1. Location of the host institution
2. Type and control of the host institution
3. Project size

The location variable has two categories: (1) urban and (2) rural. A project is classified as urban if the host institution is located in a Metropolitan Statistical Area (MSA), as defined by the U.S. Bureau of the Census. The type and control variable has three categories: (1) public, four-year; (2) private, four-year, and (3) two-year. Type and control was ascertained from the 1990-91 Integrated Postsecondary

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[2]Three of the projects in the sample are "backups," selected randomly from the same strata as three originally selected projects for which it was determined that random assignment would be inappropriate. Two of the three originally selected projects were operating under special administrative provisions, and the third project had, for several years, been unable to fill all available openings. These three projects that were replaced by backups are included in the universe counts in Table A.1.

TABLE A.1

DISTRIBUTION OF UPWARD BOUND PROJECTS: IMPACT STUDY

Stratum	Number of Projects		
		Sample	
	Universe	Selected	Respondents[1]
Urban: four-year, public			
Small:			
African American[2]	14	2	2
Latino	4	1	1
Other	7	1	1
Medium:			
Asian	5	2	2
Native American	2	1	1
Latino	9	2	2
Other	56	1	1
Large:			
African American	25	3	3
Latino	6	3	3
White	2	1	1
Other	6	1	1
Urban: four-year, private			
Small:			
African American	8	1	1
Other	5	1	1
Medium:			
Asian	4	1	1
African American	38	3	3
Latino	3	2	2
Other	5	1	1
Large:			
Asian	2	1	1
African American	22	5	3
Other	3	1	1

TABLE A.1 (*continued*)

Stratum	Number of Projects		
		Sample	
	Universe	Selected	Respondents[1]
Urban: two-year			
Small:			
Native American	1	1	1
African American	9	3	3
Latino	3	1	1
Other	5	1	1
Medium:			
Asian	2	1	1
African American	10	3	3
Other	4	1	1
Large	3	1	1
Rural: four-year, public			
Small:			
White	6	1	1
Other	6	1	1
Medium:			
Native American	7	3	2
Latino	4	1	1
Other	30	1	1
Large:			
African American	5	1	1
Other	10	2	2
Rural: four-year, private			
Small	7	1	1
Medium	14	2	2
Large	4	1	1

TABLE A.1 (*continued*)

Stratum	Number of Projects		
		Sample	
	Universe	Selected	Respondents[1]
Rural: two-year			
Small:			
African American	4	2	2
White	5	1	1
Other	6	1	1
Medium:			
African American	5	1	1
White	8	2	2
Other	5	1	1
Large:			
White	3	1	1
Other	3	1	1
Total	395	70	67

[1]Respondents are projects in which random assignment was carried out.

[2]At least 50 percent of the students served by "African American projects" are classified as African American according to the 1990-91 Upward Bound performance reports. Native American, Latino, and white projects are similarly defined. (Native American includes Alaskan Native.) For Asian projects, at least 25 percent of the students served are classified as Asian or Pacific Islander.

Education Data System (IPEDS) Institutional Characteristics file. The project size variable has three categories: (1) small (60 or fewer students), (2) medium (61 to 99 students), and (3) large (100 or more students). Enrollment figures were obtained from the 1990-91 Upward Bound performance reports.

Although some strata are defined entirely in terms of the location, type and control, and project size variables, many strata are defined by also taking into account projects' racial/ethnic compositions. At least 25 percent of the students served by "Asian projects" are classified as Asian or Pacific Islander. For a Native American (including Alaskan Native), African American, Latino, or white project, at least 50 percent of the students served are classified as members of the specified racial/ethnic group. Data on race/ethnicity were obtained from Upward Bound performance reports.

When possible, projects were sampled proportionately by racial/ethnic composition within classifications based on the other three stratifying variables. Thus, differences by racial/ethnic composition in the overall rates at which projects were sampled are largely due to disproportionate sampling by, mainly, size and type and control. Small projects, large projects, and projects hosted by two-year postsecondary institutions were oversampled to provide adequate sample sizes for subgroup analyses.

2. Second Stage Sampling: Selection of Students

For each project selected in the first stage, we identified its main recruiting period(s)--typically spring 1993, fall 1993, or both--that fell during the student sample intake period for the impact study (roughly October 1992 to March 1994). All eligible students applying to Upward Bound during a project's main recruiting period(s) were selected with certainty for the baseline impact study sample and subject to random assignment to treatment (Upward Bound) and control groups. The exceptions were students designated as "exempt" from random assignment and students who could have been

randomly chosen as "givebacks." We discuss these exceptions after describing how random assignment was conducted.

a. Random Assignment

When a project had completed recruiting for a given recruiting period, we selected eligible applicants at random to fill all available program openings. Eligible applicants not selected for Upward Bound and assigned to the treatment group were assigned to the control group or, more accurately, to a waiting list that could be used to fill certain future program openings. The waiting list and students selected from it, so-called "post-initial treatments (PITs)," are discussed in the next section.[3]

At least one round of random assignment was conducted in each of 67 projects.[4] In 17 projects with more than one recruiting period, there were two or more rounds of random assignment. We conducted a total of 87 rounds of random assignment.

Many Upward Bound project directors were concerned that the element of chance introduced by random assignment could severely unbalance the student composition of their programs. For example, it would be possible for an entire cohort of students to be from just one target school or all female.

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[3]As we discuss later, students designated as post-initial treatments will not necessarily be members of the treatment group for baseline or follow-up analyses.

[4]As indicated in Table A.1, random assignment was not carried out in 3 of the 70 projects selected for the sample. The stated policy of one of those three projects was to serve all eligible applicants. Although not policy, the practice of another project was also to serve all eligible applicants because there were few eligible students attending the project's target schools--just enough students to fill program openings, leaving none to form a control group. The third project had its funding cut and had no openings for new students. These three projects could not be replaced by backups even though, as noted earlier, three other projects in which random assignment could not be carried out had been replaced. Backups could not be selected because random assignment was determined to be infeasible only after it had been announced that no additional projects would be selected for the impact study. Failure to carry out random assignment in originally selected projects may introduce bias of unknown direction and magnitude into sample estimates.

The former outcome could have seriously damaged relationships with target schools whose students were not selected while the latter might have hampered program operations if there were not enough dormitory rooms available for females during the summer session. Therefore, project directors were allowed to specify random assignment strata and, subject to there being enough eligible applicants, to allocate available program openings across the strata to obtain the desired mix of students.[5] In all, there were 339 random assignment strata. Within a given stratum, random assignment was conducted as described before. The eligible applicants in the stratum were chosen at random to fill the available openings in the stratum. Students not picked for Upward Bound were assigned to the control group/waiting list.

b. Exemptions, Givebacks, and PITs

Exemptions. At the request of Upward Bound project directors, a very small number of students applying to Upward Bound were exempt from random assignment because it was determined that allowing the assignment of such students to be subject to the vagaries of chance could be unusually or permanently disruptive to normal program operations. For example, if a project and a local child protective services agency had a prior agreement that all eligible students referred by the agency would be accepted into Upward Bound, that agreement was not violated for the evaluation, and students referred during the sample intake period were exempt from random assignment. Strict policies of treating applicants from the same family the same were also honored and accounted for a few exemptions. For instance, one of a pair of twins applying to a project with such a policy was exempt from random assignment while the other twin was subject to random assignment. The exempt twin

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[5]Stratification was also needed in some instances to ensure that a project did not violate the federal requirement that two-thirds of the project's students be both low income and potential first generation college. For projects with multiple rounds of random assignment, each round had its own set of strata.

would be allowed to participate in Upward Bound only if the nonexempt twin was randomly assigned to the treatment group. All exempt students are nonresearch cases, meaning they were excluded from all future data collection activities and from all analyses.

Givebacks. Some projects recruited many more eligible applicants than were needed to fill available program openings and form a control group of adequate size. In such instances, we randomly selected students from the control group and "gave them back" to the projects immediately after random assignment. These givebacks could be selected by the projects to fill program openings when the impact study student intake period had ended. Although subject to random assignment, givebacks are not part of the baseline or follow-up samples. All 97 givebacks (distributed across 11 projects) are nonresearch cases.

PITs. After being selected for Upward Bound, some students never enter the program. Other students enter, but leave before completing the program. Therefore, Upward Bound projects typically maintain waiting lists of students so that program openings can be filled without having to either mount a full-scale recruiting effort or wait until the next recruiting period.

During the sample intake period for the impact study, projects were not allowed to have their own waiting lists--all nonexempt applicants were subject to random assignment.[6] To enable projects to maintain full enrollment under such conditions, we assigned students not selected for Upward Bound to an evaluation waiting list, rather than a strict control group. Students could be randomly selected from the evaluation waiting list to fill program openings, although such use of the waiting list was subject to time and size restrictions. Specifically, students could not be selected off the waiting list after a certain date, typically the start of the next recruiting period. Also, for a given random

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[6]Even students who applied to Upward Bound and were placed on a project's waiting list prior to the sample intake period for the study were generally subject to random assignment. The only exceptions were students who had been previously promised admission when openings became available. Such students were among the small number of exemptions.

assignment stratum, a student could not be selected from the waiting list if the selection of a student reduced the number of students remaining on the waiting list to less than about two-thirds the number of students originally assigned to the treatment group. Students randomly selected from the evaluation waiting list are designated as PITs. The next section discusses how PITs are used in baseline and first follow-up analyses.

B. WEIGHTING

Students were assigned sample weights that we have used in analyzing impact study data. A student's weight indicates how many students in the universe she/he represents. A student with a weight of four represents herself/himself and three other students that were not selected for the sample (or did not respond to a survey questionnaire).

Weighting has three purposes. First, weighting ensures that the sample "weights up" to the universe, producing correct totals (subject to sampling variability).[7] Second, for purposes of estimation, weighting "undoes" the effects of disproportionate sampling so that two strata with the same number of students in the universe are counted equally even if they have different numbers of students in the sample. Third, weighting adjusts for nonresponse.

In the next two sections, we describe how we assigned baseline and first follow-up weights, respectively. To exclude exemptions and givebacks from all analyses, we assigned them zero baseline and first follow-up weights. In contrast, all PITs were included in baseline analyses and received nonzero baseline weights. Whether a PIT received a nonzero first follow-up weight depended on when that student was selected off the evaluation waiting list, as discussed below.

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[7]For example, without weighting, a total estimated from a simple one-in-two random sample would, on average, fall short of the true (population) total by 50 percent.

1. Baseline Weights

We assigned nonzero baseline weights to 3,028 students--all nonexempt students except givebacks--divided into 1,479 treatments and 1,549 controls.[8] A student's baseline weight is

$$w = \frac{1}{project\ selection\ probability} \times \frac{(number\ of\ applicants)_s}{(number\ of\ applicants\ -\ number\ of\ givebacks)_s},$$

where s indexes the student's random assignment stratum. This baseline weight is the inverse of the student's probability of being selected for the baseline sample. That selection probability is

$$p = project\ selection\ probability \times \frac{(number\ of\ applicants\ -\ number\ of\ givebacks)_s}{(number\ of\ applicants)_s}$$

The first term on the right side of this last expression is the selection probability for the project to which the student applied (adjusted for nonresponse, that is, failure to carry out random assignment). The second term is the selection probability for the student *conditional* on the student's project having been selected and random assignment carried out. In other words, the two terms are, respectively, the first and second stage selection probabilities. Their product gives the student's overall (unconditional) selection probability.[9]

The first and second stage selection probabilities are easy to calculate. The first stage probability, the "project selection probability" in the last expression, is the number of projects in which random

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[8]For the baseline sample, students were designated as treatments or controls based on their initial random assignment status. Students initially selected for Upward Bound are treatments, while students initially placed on the evaluation waiting list, including students who later became PITs, are controls.

[9]In the expressions in the text, "number of applicants" is, more precisely, the number of nonexempt eligible applicants. Also, the number of applicants minus the number of givebacks is the number of treatments plus the number of controls.

assignment was carried out--given in the last column of numbers in Table A.1--divided by the number of projects in the universe--given in the first column of numbers in Table A.1. This ratio is calculated within a project's first stage sampling stratum. The second stage selection probability equals one for all but 239 students who applied to the 11 projects that received givebacks. Although they applied to projects that received givebacks, these 239 students are not themselves givebacks and, therefore, received nonzero baseline weights.[10]

Two simple examples illustrate how we calculated baseline weights. For an applicant to a large, rural project hosted by a private, four-year university, the project selection probability is 1/4. (According to Table A.1, random assignment was carried out in 1 of the 4 large, rural projects hosted by private, four-year universities.) If there were 7 other applicants (8 applicants in all) and no givebacks in the student's random assignment stratum, the second stage selection probability equals 1, and the overall selection probability equals $1/4 \times 1 = 1/4$. Therefore, the student's baseline weight is 4 (the inverse of 1/4), implying that the student represents herself/himself and three other students who applied to projects in which random assignment was not carried out (because those projects were never selected for the impact study sample). Alternatively, if there were 4 givebacks instead of none, the second stage selection probability equals 4/8, and the overall selection probability equals $1/4 \times 4/8 = 1/8$. Then, the student's baseline weight is 8. We can think of the selected student as representing herself/himself, one giveback, and six other students (two applicants to each of three projects not selected for the impact study sample).

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[10]We also created baseline weights for comparing treatments and controls (see the results in Technical Appendix B). For treatments, these "comparison baseline weights" equal the regular baseline weights described in the text. For controls, the comparison weights equal the regular weights multiplied by the ratio of treatments to controls. This ratio was calculated within random assignment strata using the regular weights. Thus, controls have the same weighted distribution across random assignment strata as treatments when comparison weights are used.

In the first example (with no givebacks), each of the 8 students in the baseline sample gets a weight of 4, and, together, the 8 students weight up to 8 × 4 = 32 students. In the second example (with 4 givebacks), each of the 4 students in the baseline sample gets a weight of 8, while each of the 4 givebacks gets a weight of 0. Together, the 4 students in the baseline sample weight up to 4 × 8 = 32 students, the correct weighted total.

Our simple examples involve applicants to a large, rural project hosted by a private, four-year university. Although random assignment was carried out in the one such project selected for the impact study sample, project selection probabilities were adjusted when necessary for nonresponse, that is, failure to carry out random assignment. For instance, the adjusted selection probability for the three large, urban, predominately African American projects hosted by four-year, private universities is 3/22. Using that selection probability, rather than 5/22, in weighting allows applicants to the 3 responding projects to represent applicants to the 2 nonresponding projects (as well as applicants to the 17 projects that were never selected in the first place).[11]

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[11]It is easy to see the consequences of not adjusting for nonresponse. Suppose all 22 large, urban, predominantly African American projects hosted by private, four-year universities had 45 applicants during the study intake period for 22 × 45 = 990 applicants in all. With random assignment carried out in 3 of the 22 projects, our baseline sample consists of 3 × 45 = 135 applicants. If we do not adjust for nonresponse, taking the project selection probability as 5/22 instead of 3/22, each student in the baseline sample has an overall selection probability of 5/22 × 1 = 5/22 (assuming no givebacks) and a baseline weight of 22/5. The 135 students in the baseline sample weight up to 135 × 22/5 = 594, which is short of the correct total (990) by the nonresponse rate (40 percent). If we adjust for nonresponse, those students weight up to 135 × 22/3 = 990, the correct total.

The nonresponse adjustment allows applicants to the responding projects to stand in for applicants to the nonresponding projects by distributing weight from the latter to the former. Had random assignment been carried out in all five projects selected for the sample, there would have been 5 × 45 = 225 students in the baseline sample, and each would have had a weight of 22/5. When random assignment was not carried out in two of the five projects, 2 × 45 =90 students did not make it into the sample. So, we divided the total weight that those 90 students would have received (90 × 22/5 = 396) equally among the 3 × 45 = 135 students who made it into the sample when random assignment was carried out in the three projects to which they applied. The adjusted weight for each of these 135 students is the unadjusted weight plus the distributive share, or 22/5 + 396/135 = 22/3 (after simplification).

2. First Follow-up Weights

As discussed in the previous section, 3,028 students received nonzero baseline weights. We wanted first follow-up data for all of these students and succeeded in obtaining at least partially completed survey questionnaires for 2,927 students--a 97 percent response rate.[12] We next discuss how we weighted students for analyses of the first follow-up data and how, in weighting students, we adjusted for the very low rate of unit nonresponse. We begin by describing how we designated students as treatments, controls, or nonresearch cases.

a. Designating Students as Treatments, Controls, or Nonresearch

Of the 3,028 students who received nonzero baseline weights, 1,524 were designated as treatments, 1,320 as controls, and 184 as nonresearch cases for first follow-up analyses. Designations were made as follows. All 1,479 students assigned to the treatment group at initial random assignment are treatments for first follow-up analyses. Likewise, all 1,320 students assigned to the evaluation waiting list at initial random assignment and not randomly selected off it (as PITs) are controls. Of the 229 PITs, 45 are treatments for first follow-up analyses, and the rest are nonresearch cases.

A PIT was designated as a treatment if two conditions were satisfied. First, the PIT had the opportunity to begin participating in Upward Bound at essentially the same time (often the same day) as the "original" treatments in the PIT's random assignment stratum. Second, the PIT did not replace a treatment who dropped out of Upward Bound (or never showed up). PITs satisfying these two conditions were designated as treatments because it is assumed that they would have been original treatments had the Upward Bound project director not underestimated the number of open slots that

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[12]Item nonresponse, that is, failure to answer individual questions, was generally not substantially higher than the 3 percent unit nonresponse rate. (Unit nonresponse is the failure to answer any questions.) Counted among the 101 nonrespondents are 3 students for whom we did not seek follow-up data because their parents had not provided consent for the students to participate in the evaluation.

were available at the initial random assignment.[13] As noted before, all other PITs were designated as nonresearch cases.

b. Preliminary Weights

Each of the 3,028 students with a nonzero baseline weight, whether designated as treatment, control, or nonresearch for first follow-up analyses, was assigned her/his baseline weight as a preliminary first follow-up weight.

c. Final Weights

To develop final weights that both facilitate comparisons of treatments and controls by weighting them up to the same totals and incorporate an adjustment for nonresponse, we calculated two sets of control totals. First, we summed the preliminary weights of all students (treatment, control, and nonresearch) in each random assignment stratum to obtain 325 control totals.[14] Second, we summed the preliminary weights of all students in each of four nonresponse "propensity classes." Each student was assigned to a propensity class based on a propensity score, which is an estimated probability that

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[13]Project directors often do not regard a slot as open until there is strong evidence that a previously enrolled student has dropped out. Therefore, rather than delaying student selection until the "last minute," some slots that were later confirmed as open were not filled in the initial random assignment.

[14]Because of nonresponse, 14 of the original 339 random assignment strata "lost" either all of their treatments or all of their controls (but never both as it turned out) and were combined with other strata. Strata were combined based on propensity scores, whose estimation is described later in the text. We combined an empty stratum with a nonempty stratum based on the similarity of students' propensity scores, as measured by the average squared difference in propensity scores between students in a given empty stratum and students in a nonempty stratum with which the empty stratum might be combined. The nonempty stratum with the lowest average squared difference was judged the most similar to the empty stratum. We did not combine strata across projects or from random assignments that occurred at widely separated points in time. Propensity scores were used to combine strata because they reflect a broad range of characteristics related to nonresponse and to the outcomes that are examined in the impact analysis. There were 16 respondents and 17 nonrespondents or nonresearch cases in the 14 empty strata.

the student was a nonrespondent.[15] Specifically, students were sorted into propensity score quartiles. The four quartiles are the four nonresponse propensity classes for which we calculated control totals by summing students' preliminary weights.

After calculating the two sets of control totals, we cross-classified the 1,481 treatment group respondents by random assignment stratum and propensity class (creating 325 × 4 = 1,300 classifications), and we used two-way raking (iterative proportional fitting) to ratio-adjust these students' preliminary weights to match the control totals and obtain final weights. Then, we repeated the procedure for the 1,265 control group respondents. Final weights for all nonrespondents and nonresearch students equal zero.[16]

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[15]The probability was obtained from a logistic regression model that related, for all 3,028 students, a binary nonresponse variable to a large set of predictor variables. This set included 49 variables in all--stratifying variables, a wide range of variables measuring student baseline characteristics, and interaction variables obtained by multiplying pairs of characteristics variables. The characteristics variables and interaction variables in the "best" logit model, that is, the model used to estimate propensity scores were selected using a forward selection procedure with a liberal inclusion criterion.

A variable included in the model should have two properties: (1) it should be a good predictor of the propensity to not respond and (2) it should be a good predictor of the outcomes of interest in the impact analysis. We tried to ensure satisfaction of the first property by using the forward selection procedure. We tried to ensure satisfaction of the second property by applying our substantive knowledge about what factors have been shown in previous studies to influence education and education-related outcomes. This led us to consider for possible inclusion in the model only some of the many variables measured at baseline. In fact, only 17 characteristics variables were considered, and 15 were included in the model by the selection procedure used. Counting stratifying and interaction variables as well as characteristics variables, we considered nearly 150 variables for inclusion in the model. After selecting a best model, we found that the estimated propensity scores effectively summarized the information contained in not only the 49 variables included in the best model, but also the full set of variables considered for inclusion in the model.

[16]The raking procedure used to obtain final weights works as follows. Suppose the preliminary weights of treatment group respondents ("students" for short) in the first two random assignment strata sum (across the four propensity classes) to 100 and 150, respectively. Then, if the control totals for these two strata are 300 and 75, we multiply the preliminary weight of every student in the first stratum by 300/100 = 3 and the preliminary weight of every student in the second stratum by 75/150 = 0.5. After such adjustments, the adjusted weights of students in both strata will sum to the respective control totals. Next, we further adjust the adjusted weights to match the control totals by propensity class. If each of the four propensity class control totals is 5,000, but students' weights after the previous adjustments sum (across all 325 random assignment strata) to 2,000, 5,000, 7,000, and

The final first follow-up weights are constructed so that the weighted distribution of treatments across random assignment strata (calculated using the final weights) is the same as the weighted distribution of eligible applicants across random assignment strata *at baseline* (calculated using baseline weights). Likewise, the weighted distribution of controls is the same as the weighted distribution of eligible applicants. Therefore, the weighted distributions of treatments and controls are the same.[17,18]

C. CALCULATING STANDARD ERRORS

Throughout this report, we present many estimates, such as percentages and means. These estimates are called "point" estimates because they are single values, as opposed to ranges of values. We can also obtain standard errors for these point estimates. As its name implies, a standard error is an estimate of the error in a point estimate, that is, an expression of our uncertainty. Typically, standard errors are used to construct "interval" estimates or "confidence intervals" that give a range of possible values. A "95-percent" confidence interval extends from two standard errors below the

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6,000, the weights of students in the first, second, third, and fourth propensity classes are multiplied by 5/2, 1, 5/7, and 5/6, respectively. Although the propensity class control totals will be matched after this last round of adjustments, students' weights may no longer sum to the random assignment stratum control totals. If not, the adjustments by random assignment stratum and, then, by propensity class are repeated until students' weights satisfy both sets of control totals. This condition was met for both treatments and controls.

[17]The distributions of treatments and controls across propensity classes are also the same, with both matching the distribution of eligible applicants.

[18]Out of concern that the study--for example, its sample-size requirements--may have distorted the composition of the eligible applicant pool, we developed an alternative set of weights. The alternative weights were constructed so that both the distribution of treatments and the distribution of controls (across either random assignment strata or propensity classes) are the same as the baseline distribution of *treatments*, rather than applicants. However, all the results presented in this report were obtained using the weights controlled to the distributions of applicants because it turned out that essentially the same results were obtained using the alternative weights controlled to the distributions of treatments.

point estimate to two standard errors above the point estimate. Thus, when we estimate from baseline data that 11 percent of eligible Upward Bound applicants do not expect to attend college and the standard error for this estimate is 2 percent, the 95-percent confidence interval runs from $11 - 2 \times 2$ to $11 + 2 \times 2$, or from 7 to 15 percent. One interpretation of this confidence interval is that if we repeated our sampling and estimation procedures 100 times (drawing a new random sample each time), about 95 percent or 95 of the 100 confidence intervals that we construct will contain the true percentage of eligible Upward Bound applicants who do not expect to attend college. That true percentage is the percentage that would have been obtained if we had surveyed all applicants in the universe, rather than a sample of 3,028 applicants.[19] In our example, we are 95-percent "confident" that the true percent of applicants who do not expect to attend college lies between 7 and 15 percent. Of course, the true percentage either does or does not lie in that range.

Instead of confidence intervals, we can derive "t-statistics," which are closely related to confidence intervals. Dividing a point estimate by its standard error gives a t-statistic. If that t-statistic is less than two in absolute value (that is, between -2 and 2), we conclude that, at the 95-percent confidence (5-percent significance) level, the point estimate is not "significantly different" from zero; in other words, the observed difference from zero may be due entirely to the element of chance introduced by sampling. Determining whether the t-statistic is less than two in absolute value is the same as determining whether the confidence interval includes zero. When the confidence interval includes zero, we are not confident that the true value is different from zero.[20]

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[19]We estimate that there were nearly 22,000 eligible students who applied to Upward Bound during the impact study sample intake period.

[20]For purposes of illustration, we have described here a "two-tailed" test, where any estimate far enough from zero--either above or below--casts doubt on the hypothesis that the true value is zero. Since the aim of most analyses presented in this report is to determine whether Upward Bound has a beneficial impact and not whether it just has some impact, good or bad, we have usually conducted "one-tailed" tests. With a one-tailed test, only an impact estimate far enough above zero supports the

To estimate error and express our uncertainty accurately, standard errors must be calculated using methods that reflect how the sample was drawn. For almost all analyses, we used SUDAAN, a computer software package that uses the Taylor series linearization method to calculate standard errors based on the user's coded description of the sample design.[21]

In using SUDAAN to calculate standard errors of impact estimates from the first follow-up sample, our description of the sample design was developed from not only our knowledge of how the sample was drawn but also an algebraic result pertaining to the variance of the difference between treatment and control group means, that is, the variance of the estimated impact.[22]

Because all the applicants, rather than a sample of applicants, to an Upward Bound project were assigned to treatment and control groups in the second stage of sampling (except for a small number of givebacks assigned to a nonresearch group in a few projects), the treatment and control group means for the project are correlated, indeed, negatively correlated. When the treatment group mean is higher than expected, the control group mean is lower than expected and vice versa. If the fraction of students assigned to the treatment group is p_T ($0 < p_T < 1$), it can be shown that the covariance between the treatment and control group means of outcome X for the project is:[23]

$$cov(\bar{X}_T, \bar{X}_C) = - \frac{p_T \; var \, \bar{X}_T}{1 - p_T} .$$

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hypothesis that Upward Bound has a beneficial impact. Any other impact estimate--including a large, but negative estimate--casts doubt on that hypothesis.

[21]To describe the relationships between project characteristics and variations in program impacts (and measure the uncertainty in our estimates characterizing those relationships), we estimated hierarchical linear models using the HLM computer program.

[22]The variance is the standard error squared.

[23]The covariance is the correlation times the standard error of each of the means.

Then, the variance of the difference between the treatment and control group means for the project is:

$$var(\bar{X}_T - \bar{X}_C) = \frac{1 + p_T}{1 - p_T} var\,\bar{X}_T + var\,\bar{X}_C$$

Since

$$var\,\bar{X}_T = (1 - p_T)\,\frac{\sigma_T^2}{n_T}$$

and

$$var\,\bar{X}_C = [1 - (1 - p_T)]\,\frac{\sigma_C^2}{n_C} = p_T\,\frac{\sigma_C^2}{n_C}$$

when we take account of the finite population corrections (fpc's), we have:[24]

$$var(\bar{X}_T - \bar{X}_C) = (1 + p_T)\,\frac{\sigma_T^2}{n_T} + p_T\,\frac{\sigma_C^2}{n_C}$$

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[24]The fpc is made because the collection of students sampled, that is, randomly assigned for each project is "not large" and the sampling is without replacement. Sampling without replacement means that a student can be randomly assigned only once. In the expressions for the variances of means, n_T and n_C are the numbers of treatments and controls, respectively. σ_T^2 is the variance of the outcome across treatments, while σ_C^2 is the variance across controls.

But, under the reasonable approximations that the experimental design is balanced ($n_T = n_C = n$, that is, $p_T = 0.5$) and the variability of outcomes is the same in the treatment and control groups ($\sigma_T^2 = \sigma_C^2 = \sigma^2$):

$$var(\bar{X}_T - \bar{X}_C) = 2\,\frac{\sigma^2}{n},$$

the result obtained when we ignore the fpc's and the correlation between the treatment and control group means. Ignoring the fpc's amounts to assuming that students were sampled (randomly assigned) with replacement in the second stage sampling. Ignoring the correlation between treatment and control group means amounts to assuming that only small fractions of applicants become treatments and controls, while most are assigned to a nonresearch group.

The last result suggested a simple way for us to estimate standard errors of impact estimates using SUDAAN. Specifically, contrary to fact, we specified that sampling at the second stage was with replacement and assumed that treatment and control group means were uncorrelated.[25]

To estimate standard errors, we also had to combine first stage sampling strata because many of them had only one Upward Bound project in which random assignment was carried out. If we had not combined strata, it would not have been possible to measure variability among projects within many strata. We combined strata that differed in terms of control (public/private) or racial/ethnic composition but never location (urban/rural) or type (four-year/two-year). There was only one instance in which strata for different size projects were combined.

Because many second stage random assignment strata only had one treatment or one control (not enough students to estimate a variance), we also had to combine those strata. We generally combined "similar" strata, for example, a stratum for males from one target school with a stratum for males from

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[25]We did specify without replacement sampling at the first stage.

another target school. We never combined strata for different projects or widely separated rounds of random assignment.

TECHNICAL APPENDIX B

COMPARISON OF UPWARD BOUND GROUP AND CONTROL GROUP BEFORE RANDOM ASSIGNMENT

TABLE B.1

COMPARISON OF UPWARD BOUND GROUP AND CONTROL GROUP
BEFORE RANDOM ASSIGNMENT

Baseline Variable	Group Means: Upward Bound	Group Means: Control	Difference
Students' educational expectations	16.8	17.2	-.4
Fathers' educational expectations	16.7	16.9	-.3
Mothers' educational expectations	16.8	17.2	-.4
How often talked with parents about:			
Courses	1.9	2.0	-.1
School activities	2.1	2.1	-.1
Studies	1.9	2.0	-.1**
Grades	2.4	2.5	-.1
Transferring to another school	.7	.7	.0
Taking ACT/SAT exam	1.0	1.1	-.1*
College plans	2.2	2.3	-.1
How often parent:			
Checked on homework	2.1	2.2	-.1
Helped with homework	1.7	1.8	-.2
Gave special privileges	1.9	1.9	.0
Limited privileges	1.7	1.7	.0
Required chores	2.6	2.6	.0
Limited TV/video time	1.5	1.4	.1
Limited time with friends	2.1	2.1	.1
Times:			
Late for school	2.6	2.8	-.2
Skipped classes	.8	.8	.0
Missed a day of school	3.9	3.7	.2
In trouble for not following school rules	1.1	1.1	.0
Put on in-school suspension	.4	.3	.1
Suspended	.2	.2	.0
Transferred for disciplinary reasons	.0	.0	.0
Arrested	.0	.0	.0
Spent time in juvenile home	.0	.0	.0

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Table B.1 (*continued*)

Baseline Variable	Group Means: Upward Bound	Group Means: Control	Difference
Parent:			
Attended school meeting	.6	.7	.0
Spoke with teachers	.7	.7	.0
Visited classes	.5	.5	.0
Attended school event	.7	.7	.0
Hours spent:			
On homework	7.2	7.5	-.3
On school-sponsored activities	4.7	5.3	-.6
Playing video games (weekdays)	.9	.8	.1
Playing video games (weekends)	1.4	1.3	.1**
Academic preparation:			
Grade point average	2.4	2.4	.0
Credits earned in science	.2	.3	-.1
Credits earned in math	.5	.6	-.1
Credits earned in English	1.0	1.1	-.1
Credits earned in foreign languages	.4	.4	.0
Credits earned in social studies	.8	.8	.0
Total academic credits earned	3.0	3.3	-.3
Credits earned in vocational education	.5	.4	.0
Credits earned in remedial math	.3	.3	.1
Credits earned in Algebra I	.4	.5	-.1
Credits earned in Geometry	.1	.1	.0**
Credits earned in Algebra II	.0	.0	.0

*Significant at the .10 level.
**Significant at the .05 level.

NOTE: Differences have been rounded.

TECHNICAL APPENDIX C

COMPUTING IMPACT ESTIMATES FOR PARTICIPANTS

Computing a simple difference-of-means for Upward Bound applicants assigned to the treatment and control groups tells us the program's impact for all applicants offered a slot in the program and, thus, the impact of being offered services. It does not tell us the impact of participating in the program, that is, the impact of accepting the offer of services. The impact estimate for all applicants selected for Upward Bound is:

$$\text{Impact (applicants)} = E(y_T) - E(y_c)$$

where $E(\cdot)$ corresponds to the expected value for a random variable (i.e., a student outcome) and the subscripts T and C refer to the treatment and control groups, respectively.

Our analysis of participation in Upward Bound shows that about 20 percent of those selected for the program chose to not participate. We can form an estimate of program impacts for participants by using the following framework. First, we express the mean (expected) value for an outcome such as academic credits earned for members of the treatment and control groups as:

$$E(y_T) = E(y_T \mid S = 1)Pr(S = 1)_T + E(y_T \mid S = 0)(1 - Pr(S = 1)_T)$$

and

$$E(y_c) = E(y_c \mid S = 1)Pr(S = 1)_c + E(y_c \mid S = 0)(1 - Pr(S = 1)_c)$$

where $Pr(S = 1)$ indicates the probability of participating in Upward Bound if offered a slot ($S = 1$ when a student showed up and $S = 0$ otherwise).

For treatment group students, we are interested in the expected value of the student outcome, given that students participated in Upward Bound when offered an opportunity to participate. For control group students, we are interested in the expected value of the student outcome, given that the

students would have participated if offered an opportunity. Using the two expressions shown above, we can express the desired quantities as:

$$E(y_T \mid S = 1) = [E(y_T) - E(y_T \mid S = 0)(1 - Pr(S = 1)_T]/Pr(S = 1)_T$$

and

$$E(y_c \mid S = 1) = [E(y_c) - E(y_c \mid S = 0)(1 - Pr(S = 1)_c]/Pr(S = 1)_c$$

Then, the impact on participants is:

$$\text{Impact (participants)} = E(y_T \mid S = 1) - E(y_c \mid S = 1)$$

If we assume that the mean for members of the treatment group who did not show up for Upward Bound is the same as the mean for members of the control group who would not have shown up if offered an opening in Upward Bound ($E(y_c \mid S = 0) = E(y_T \mid S = 0)$), and the proportion of no-shows would be the same for the two groups ($Pr(S = 1)_c = Pr(S = 1)_T$), then we can estimate the impact for participants as:

$$\text{Impact (participants)} = [E(y_T) - E(y_c)]/Pr(S = 1)_T$$

$$= \text{Impact (applicants)}/Pr(S = 1)_T$$

If we assume the participation rate remains constant in repeated samples, then we can write the standard error of the impact estimate as:

$$SE\ [\text{Impact (participants)}] = SE\ [\text{Impact (applicants)}]/Pr(S = 1)_T$$

When we form a t-statistic the adjustment factor, $Pr(S=1)_T$, cancels out of the expression and we are left with the same t-statistic used for the impact estimate based on *all students* selected for Upward Bound.

TECHNICAL APPENDIX D

NO-SHOW RATES FOR SELECTED SUBGROUPS

TABLE D.1

NO-SHOW RATES

	Show-up Rate (%)	p-Value for χ^{2} [a]
Sex:		0.24
Female	80	
Male	85	
Race:		0.36
African American	76	
Hispanic	91	
Asian	93	
White	83	
Native American	79	
Other race	87	
Grade level:		0.03
7th	87	
8th	83	
9th	80	
10th	75	
Low income/first generation:		0.00
Low income and first generation	82	
Low income only	97	
First generation only	73	
Upward Bound project director's rating:		0.51
Most likely to be selected	84	
Somewhat likely to be selected	77	
Least likely to be selected	87	
Summer work for pay:		0.21
Worked for pay last summer	80	
Did not work for pay last summer	82	
College plans:		0.15
Plan to complete at least a college degree	81	
Do not plan to complete a college degree	78	

[a]This shows the probability of obtaining such a high value of χ^{2} under the hypothesis that each group of students has the same show-up rate. Probabilities of less than .10 indicate that one or more categories has a smaller- or larger-than-expected percentage of "show-ups."

TECHNICAL APPENDIX E

DECOMPOSING THE IMPACT OF UPWARD BOUND ON ACADEMIC COURSE TAKING

We can decompose the average number of credits earned for the treatment and control groups into two parts by looking at the problem as one of computing a weighted average. The average gain in credits for members of the treatment group can be expressed as:

$$\bar{y}_T = (1 - \Pr(\text{credits earned} > 0 \mid \text{Treatment})) \times (0 \text{ credits}) + \Pr(\text{credits earned} > 0 \mid \text{Treatment}) \times E\ (y_T \mid \text{credits earned} > 0, \text{Treatment})$$

where $E(y_T)$ corresponds to the average number of credits earned by members of the treatment group and Pr (·) refers to the probability of earning more than 0 academic credits. For the control group we can also write:

$$\bar{y}_c = (1 - \Pr(\text{credits earned} > 0 \mid \text{Control})) \times (0 \text{ credits}) + \Pr(\text{credits earned} > 0 \mid \text{Control}) \times E\ (y_c \mid \text{credits earned} > 0, \text{Control})$$

Since the first term in each expression for the average gain in the treatment and control groups equals zero, we can form an estimate of program impact as:

$$\text{Impact} = [\Pr(\text{credits earned} > 0 \mid \text{Treatment}) \times E\ (y_T \mid \text{credits earned} > 0, \text{Treatment})] - [\Pr(\text{credits earned} > 0 \mid \text{Control}) \times E\ (y_c \mid \text{credits earned} > 0, \text{Control})]$$

From this expression we can see that the impact estimate is a function of the probability of participating in an academic curriculum (i.e., earning some credit in one or more academic courses) and the expected number of credits students earn if they participate in this curriculum. We can simulate different levels of participation in an academic curriculum by substituting alternative values for the probability of earning credits in an academic curriculum for members of the treatment and control groups. For example, to help us understand differences in course taking by Hispanic and African American youths, we can perform the following steps:

1. For Hispanic students, compute the impact of Upward Bound on total academic credits earned (.99 × 6.95 - .87 × 5.63 = 1.99)

2. Compute the impact for Hispanics assuming they have the same chances of participating in an academic curriculum as African American students (.92 × 6.95 - .96 × 5.63 = .99)

3. Adjust these impacts by the program participation rate (one minus the no-show rate of .08 for Hispanics). This produces impact estimates of 2.18 (= 1.99 × 1.10) and 1.09 (= .99 × 1.10) under the two conditions. This means that the impact for Hispanic students would be reduced by half if they had participated in an academic curriculum with the same relative frequency as African American youth in Upward Bound and the control group.

TECHNICAL APPENDIX F

DESCRIPTIVE STATISTICS ON UPWARD BOUND APPLICANTS

TABLE F.1

DESCRIPTIVE STATISTICS ON UPWARD BOUND APPLICANTS

Characteristics	Mean
Demographics:	
Female	0.70
Male	0.30
Grade level	8.41
White	0.21
Black	0.50
Hispanic	0.22
Native American	0.03
Asian	0.03
Other race/ethnicity	0.01
Attitudes (reporting agree or strongly agree):	
Feel good about self	0.94
No control over life	0.19
Good luck more important	0.10
Person of worth	0.94
Do things as well as others	0.94
Can't get ahead	0.24
Plans hardly ever work out	0.17
Satisfied with myself	0.89
Feel useless at times	0.35
Think I am no good at all	0.27
Can make plans work	0.85
Not much to be proud of	0.12
Chance and luck important	0.39
Expectations (years of schooling):	
Student's educational expectations	17.00
Behavior (reported as number of times):	
Late for school	2.69
Skipped classes	0.71
Missed a day of school	3.66
Not following school rules	1.10
In-school suspension	0.32
Probation from school	0.21
Transferred, disciplinary reasons	0.02
Arrested	0.03
Time in juvenile home	0.03
Work for pay:	
Work for pay in the last school year	0.11
Work for pay last summer	0.27

TABLE F.1

DESCRIPTIVE STATISTICS ON UPWARD BOUND APPLICANTS

Characteristics	Mean
Family resources and structure:	
Have in home: place for study	0.67
Have in home: daily newspaper	0.55
Have in home: regular magazine	0.53
Have in home: encyclopedia	0.65
Have in home: atlas	0.46
Have in home: dictionary	0.96
Have in home: typewriter/word processor	0.58
Have in home: computer	0.21
Have in home: more than 50 books	0.69
Have in home: VCR	0.87
Have in home: pocket calculator	0.89
Have in home: room of your own	0.73
Same household: father	0.45
Same household: other male guardian	0.12
Same household: mother	0.90
Same household: other female guardian	0.03
Same household: brothers	0.52
Same household: sisters	0.53
Same household: grandparents	0.11
Same household: other relatives	0.10
Same household: non-relatives	0.03
Reasons applied for Upward Bound:	
Improve grades	0.98
Improve self-confidence	0.93
Chances getting into college	0.99
Prepare for college entrance exam	0.97
Identify financial aid sources	0.88
Select an appropriate college	0.91
Something else	0.21

TECHNICAL APPENDIX G

PROGRAM IMPACTS FOR SUBGROUPS

TABLE G.1

UPWARD BOUND IMPACTS, BY RACE/ETHNICITY

Outcome	All Students			Test of No Interaction[1]	Participants		
	Black	White	Hispanic		Black	White	Hispanic
Academic preparation:							
Grade point average	0.02	0.06	0.02		0.03	0.07	0.02
Credits earned in science	(0.01)	0.10 *	0.34 **		(0.01)	0.12 *	0.38 **
Credits earned in math	0.04	0.06	0.36 **	*	0.05	0.07	0.40 **
Credits earned in English	0.10 *	0.16	0.44 **	**	0.13 *	0.19	0.49 **
Credits earned in foreign languages	(0.02)	0.04	0.47 **	**	(0.03)	0.05	0.52 **
Credits earned in social studies	0.08	0.02	0.37 **	**	0.10	0.02	0.41 **
Total academic credits earned	0.19	0.37	1.99 **	**	0.25	0.44	2.20 **
Credits earned in vocational education	0.02	(0.10)	0.70 **	**	0.03	(0.12)	0.77 **
Credits earned in remedial math	0.05	0.01	0.01		0.07	0.01	0.01
Educational expectations and attitudes:							
Students'	0.40	0.19 *	(0.45)		0.52	0.23 *	(0.50)
Fathers'	0.48 **	0.05	(0.09)		0.62 **	0.06	(0.10)
Mothers'	0.26 *	0.07	0.29		0.34 *	0.08	0.32
Self-esteem	(0.01)	0.07	(0.07)		(0.01)	0.08	(0.08)
Locus of control	(0.05)	0.03	(0.01)		(0.07)	0.04	(0.01)
How often talked with parents:							
About courses	(0.02)	(0.20)	0.00		(0.03)	(0.24)	0.00
About school activities	(0.09)	(0.14)	(0.20)		(0.12)	(0.17)	(0.22)
About studies	(0.02)	(0.13)	(0.12)		(0.03)	(0.16)	(0.13)
About grades	0.02	(0.16)	(0.08)	*	0.03	(0.19)	(0.09)
About transferring to another school	0.15 **	0.00	0.00		0.20 **	0.00	0.00
About taking or preparing for the ACT/SAT	0.01	(0.10)	(0.22)		0.01	(0.12)	(0.24)
About college	0.01	(0.19)	0.14 *		0.01	(0.23)	0.15 *
How often parents:							
Checked on homework	0.08	(0.04)	0.46 **	**	0.10	(0.05)	0.51 **
Helped with homework	0.17 *	(0.02)	0.45 *		0.22 *	(0.02)	0.50 *
Gave special privileges for good grades	0.00	(0.03)	(0.05)		0.00	(0.04)	(0.06)
Limited privileges because of poor grades	(0.14)	0.07	0.01		(0.18)	0.08	0.01
Required chores around the house	(0.08)	(0.10)	0.06		(0.10)	(0.12)	0.07
Limited time watching TV or playing video games	0.06	(0.06)	(0.26)		0.08	(0.07)	(0.29)
Limited time with friends	(0.08)	(0.10)	(0.14)		(0.10)	(0.12)	(0.15)
Attended a school meeting	0.03	0.07	0.05		0.04	0.08	0.06
Spoke with teachers	0.03	(0.07)	0.01		0.04	(0.08)	0.01
Visited classes	(0.06)	(0.06)	(0.20)		(0.08)	(0.07)	(0.22)
Attended a school event	(0.08)	(0.02)	(0.20)		(0.10)	(0.02)	(0.22)
Misbehavior:							
Times late for school	0.57 **	0.83 *	(0.01)		0.74 **	0.99 *	(0.01)
Times skipped classes	0.15 **	0.03	(0.33)		0.20 **	0.04	(0.36)
Times missed a day of school	0.16	0.42 *	(0.12)		0.21	0.50 *	(0.13)
Times in trouble for not following school rules	0.39 **	(0.07)	(0.39)	**	0.51 **	(0.08)	(0.43)
Times put on in-school suspension	0.10	0.03	(0.32)	**	0.13	0.04	(0.35)
Times suspended	(0.01)	(0.03)	0.04		(0.01)	(0.04)	0.04
Times transferred for disciplinary reasons	0.01	(0.01)	(0.01)		0.01	(0.01)	(0.01)
Times arrested	0.01	(0.02)	(0.02)		0.01	(0.02)	(0.02)
Times spent time in juvenile home	0.04	(0.08)	(0.06)		0.05	(0.10)	(0.07)

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

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TABLE G.2

UPWARD BOUND IMPACTS, BY EDUCATIONAL EXPECTATIONS

Outcome	All Students			Participants	
	Low Expectations	High Expectations	Test of No Interaction[1]	Low Expectations	High Expectations
Academic preparation:					
Grade point average	0.09	0.02		0.12	0.02
Credits earned in science	0.36 **	0.12 **		0.46 **	0.15 **
Credits earned in math	0.46 **	0.10 **	**	0.59 **	0.12 **
Credits earned in English	0.66 **	0.06	**	0.84 **	0.07
Credits earned in foreign languages	0.32 **	0.09 *		0.41 **	0.11 *
Credits earned in social studies	0.62 **	0.06	**	0.79 **	0.07
Total academic credits earned	2.43 **	0.41 **	**	3.11 **	0.50 **
Credits earned in vocational education	0.18	0.18 *		0.23	0.22 *
Credits earned in remedial math	0.15 **	(0.01)	**	0.19 **	(0.01)
Educational expectations and attitudes:					
Students'	0.15	0.06		0.19	0.07
Fathers'	0.94 **	0.01	**	1.20 **	0.01
Mothers'	0.71 **	0.09	**	0.91 **	0.11
Self-esteem	0.03	0.00		0.04	0.00
Locus of control	(0.01)	(0.01)		(0.01)	(0.01)
How often talked with parents:					
About courses	(0.36)	0.04		(0.46)	0.05
About school activities	(0.71)	0.00	*	(0.91)	0.00
About studies	(0.57)	(0.01)		(0.73)	(0.01)
About grades	(0.40)	0.03		(0.51)	0.04
About transferring to another school	(0.29)	0.20 **	*	(0.37)	0.25 **
About taking or preparing for the ACT/SAT	(0.41)	0.04		(0.52)	0.05
About college	(0.42)	0.05	*	(0.54)	0.06
How often parents:					
Checked on homework	(0.07)	0.17 **	**	(0.09)	0.21 **
Helped with homework	(0.24)	0.28 *	*	(0.31)	0.34 *
Gave special privileges for good grades	(0.46)	0.09 *	**	(0.59)	0.11 *
Limited privileges because of poor grades	(0.20)	(0.04)		(0.26)	(0.05)
Required chores around the house	(0.05)	(0.01)		(0.06)	(0.01)
Limited time watching TV or playing video games	(0.44)	0.04	*	(0.56)	0.05
Limited time with friends	0.11	(0.16)		0.14	(0.20)
Attended a school meeting	0.00	0.03		0.00	0.04
Spoke with teachers	(0.10)	0.01	*	(0.13)	0.01
Visited classes	(0.13)	(0.07)		(0.17)	(0.09)
Attended a school event	(0.14)	(0.08)		(0.18)	(0.10)
Misbehavior:					
Times late for school	0.85	0.32 **		1.09	0.39 **
Times skipped classes	0.34	0.12		0.44	0.15
Times missed a day of school	0.66 **	0.04	*	0.84 **	0.05
Times in trouble for not following school rules	(0.05)	0.28 **		(0.06)	0.34 **
Times put on in-school suspension	(0.12)	0.11 *	*	(0.15)	0.14 *
Times suspended	0.03	0.02		0.04	0.02
Times transferred for disciplinary reasons	(0.02)	0.01		(0.03)	0.01
Times arrested	0.03	0.00		0.04	0.00
Times spent time in juvenile home	(0.06)	0.02		(0.08)	0.02

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

TABLE G.3

UPWARD BOUND IMPACTS, BY LOW INCOME/FIRST GENERATION STATUS

Outcome	All Students: First Generation/ Low Income	All Students: First Generation Only	All Students: Low Income Only	All Students: Test of No Interaction[1]	Participants: First Generation/ Low Income	Participants: First Generation Only	Participants: Low Income Only
Academic preparation:							
Grade point average	(0.01)	0.19 **	(0.01)		(0.01)	0.26 **	(0.01)
Credits earned in science	0.24 **	(0.12)	(0.35)	*	0.29 **	(0.16)	(0.36)
Credits earned in math	0.12 *	0.24 **	(0.27)		0.15 *	0.33 **	(0.28)
Credits earned in English	0.29 **	(0.01)	(0.08)		0.35 **	(0.01)	(0.08)
Credits earned in foreign languages	0.11	0.15 *	0.07		0.13	0.21 *	0.07
Credits earned in social studies	0.25 **	(0.06)	(0.12)		0.31 **	(0.08)	(0.12)
Total academic credits earned	1.01 **	0.21	(0.77)		1.23 **	0.29	(0.80)
Credits earned in vocational education	0.21	0.23 *	(0.02)		0.26	0.32 *	(0.02)
Credits earned in remedial math	0.08 **	(0.13)	(0.09)		0.10 **	(0.18)	(0.09)
Educational expectations and attitudes:							
Students'	0.16 **	0.30	0.21		0.20 **	0.41	0.22
Fathers'	0.17 *	0.45 *	0.51		0.21 *	0.62 *	0.53
Mothers'	0.11	0.74 *	0.35		0.13	1.01 *	0.36
Self-esteem	(0.01)	0.07	(0.17)		(0.01)	0.10	(0.18)
Locus of control	(0.05)	0.10	0.01		(0.06)	0.14	0.01
How often talked with parents:							
About courses	(0.08)	0.00	0.19		(0.10)	0.00	0.20
About school activities	(0.11)	(0.16)	(0.19)		(0.13)	(0.22)	(0.20)
About studies	(0.10)	(0.02)	0.04		(0.12)	(0.03)	0.04
About grades	(0.10)	0.29 **	(0.21)	*	(0.12)	0.40 **	(0.22)
About transferring to another school	0.08 *	0.26 *	(0.30)		0.10 *	0.36 *	(0.31)
About taking or preparing for the ACT/SAT	(0.05)	(0.15)	0.02		(0.06)	(0.21)	0.02
About college	(0.06)	0.26 **	(0.13)	**	(0.07)	0.36 **	(0.14)
How often parents:							
Checked on homework	0.10	0.28 **	(0.29)	**	0.12	0.38 **	(0.30)
Helped with homework	0.08	0.70 **	(0.01)	**	0.10	0.96 **	(0.01)
Gave special privileges for good grades	0.00	0.02	(0.36)		0.00	0.03	(0.37)
Limited privileges because of poor grades	0.01	(0.30)	(0.33)		0.01	(0.41)	(0.34)
Required chores around the house	(0.13)	0.20 **	0.15		(0.16)	0.27 **	0.16
Limited time watching TV or playing video games	(0.06)	0.11	(0.15)		(0.07)	0.15	(0.16)
Limited time with friends	(0.12)	0.00	(0.03)		(0.15)	0.00	(0.03)
Attended a school meeting	0.02	0.07	0.12		0.02	0.10	0.12
Spoke with teachers	(0.04)	0.18	0.01		(0.05)	0.25	0.01
Visited classes	(0.06)	(0.17)	(0.18)		(0.07)	(0.23)	(0.19)
Attended a school event	(0.10)	(0.02)	(0.09)		(0.12)	(0.03)	(0.09)
Misbehavior:							
Times late for school	0.55 **	0.17	0.54		0.67 **	0.23	0.56
Times skipped classes	0.04	0.22	(0.76)		0.05	0.30	(0.79)
Times missed a day of school	0.45 **	(0.88)	(0.96)	*	0.55 **	(1.21)	(1.00)
Times in trouble for not following school rules	0.15 **	0.19	(0.23)		0.18 **	0.26	(0.24)
Times put on in-school suspension	(0.03)	0.08	0.17		(0.04)	0.11	0.18
Times suspended	(0.01)	0.03	0.04		(0.01)	0.04	0.04
Times transferred for disciplinary reasons	0.00	(0.01)	0.04		0.00	(0.01)	0.04
Times arrested	0.01	(0.03)	(0.04)		0.01	(0.04)	(0.04)
Times spent time in juvenile home	(0.03)	(0.01)	0.46 *		(0.04)	(0.01)	0.48 *

* Significant at the .10 level.

** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

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TABLE G.4

UPWARD BOUND IMPACTS, BY GPA

Outcome	Average GPA = D	Average GPA = C	Test of No Interaction[1]
Academic preparation:			
Grade point average	(0.00)	0.07	
Credits earned in science	0.30 **	0.22 **	
Credits earned in math	0.32 *	0.22 *	
Credits earned in English	0.38 **	0.38 **	
Credits earned in foreign languages	0.25 **	0.16 *	
Credits earned in social studies	0.08	0.29 **	*
Total academic credits earned	1.32 **	1.24 **	
Credits earned in vocational education	0.05	0.23	
Credits earned in remedial math	0.02	0.09 **	*
Educational expectations and attitudes:			
Students'	0.04	0.19	
Fathers'	(0.97)	0.02	
Mothers'	1.18 **	0.27 *	
Self-esteem	0.07	(0.07)	
Locus of control	0.04	(0.03)	
How often talked with parents:			
About courses	(0.27)	(0.18)	
About school activities	(0.12)	(0.29)	
About studies	(0.73)	(0.15)	
About grades	(0.39)	(0.37)	
About transferring to another school	0.62 **	0.09	**
About taking or preparing for the ACT/SAT	0.15	(0.02)	
About college	(0.02)	(0.09)	
How often parents:			
Checked on homework	0.23	0.04	
Helped with homework	0.21	0.14	
Gave special privileges for good grades	(0.00)	0.01	
Limited privileges because of poor grades	(0.66)	(0.01)	**
Required chores around the house	(0.41)	0.06	**
Limited time watching TV or playing video games	0.54	(0.27)	
Limited time with friends	(0.12)	(0.19)	
Attended a school meeting	(0.23)	(0.05)	**
Spoke with teachers	(0.16)	(0.19)	**
Visited classes	(0.22)	(0.22)	
Attended a school event	0.20 *	(0.14)	
Misbehavior:			
Times late for school	(0.86)	0.62 **	*
Times skipped classes	(0.71)	0.49 **	
Times missed a day of school	(0.60)	0.01	
Times in trouble for not following school rules	0.41	0.12	
Times put on in-school suspension	0.38	(0.01)	
Times suspended	(0.05)	0.07	
Times transferred for disciplinary reasons	0.04	0.05 **	
Times arrested	(0.26)	(0.07)	
Times spent time in juvenile home	0.12	(0.05)	

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

TABLE G.5

UPWARD BOUND IMPACTS, BY SEX

Outcome	Male	Female	Test of No Interaction[1]
Academic preparation:			
Grade point average	0.02	0.03	
Credits earned in science	0.24 **	0.13 **	
Credits earned in math	0.12 *	0.13 **	
Credits earned in English	0.31 **	0.18 **	
Credits earned in foreign languages	0.16 **	0.11	
Credits earned in social studies	0.17	0.18 **	
Total academic credits earned	1.00 **	0.73 **	
Credits earned in vocational education	0.19	0.20 *	
Credits earned in remedial math	0.10	0.01	
Educational expectations and attitudes:			
Students'	0.10	0.22 *	
Fathers'	0.55 **	0.09	
Mothers'	0.38 *	0.18	
Self-esteem	(0.01)	0.00	
Locus of control	(0.13)	0.03	
How often talked with parents:			
About courses	0.03	(0.10)	
About school activities	(0.10)	(0.14)	
About studies	(0.13)	(0.06)	
About grades	(0.08)	(0.01)	
About transferring to another school	0.02	0.12 *	
About taking or preparing for the ACT/SAT	(0.29)	0.04	
About college	(0.24)	0.09	
How often parents:			
Checked on homework	0.20 **	0.10	
Helped with homework	0.21 *	0.17	
Gave special privileges for good grades	(0.08)	0.01	
Limited privileges because of poor grades	0.07	(0.12)	
Required chores around the house	(0.13)	(0.04)	
Limited time watching TV or playing video games	(0.04)	(0.04)	
Limited time with friends	0.03	(0.16)	
Attended a school meeting	0.02	0.04	
Spoke with teachers	(0.06)	0.03	
Visited classes	(0.02)	(0.10)	
Attended a school event	(0.05)	(0.11)	
Misbehavior:			
Times late for school	0.31	0.53 **	
Times skipped classes	0.35 *	(0.12)	
Times missed a day of school	0.75 **	(0.11)	
Times in trouble for not following school rules	0.51 **	(0.05)	*
Times put on in-school suspension	0.06	(0.03)	
Times suspended	0.10	(0.05)	
Times transferred for disciplinary reasons	0.00	0.00	
Times arrested	(0.06)	0.02 *	
Times spent time in juvenile home	0.01	(0.02)	

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

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TABLE G.6

UPWARD BOUND IMPACTS, BY PROJECT DIRECTOR RATING

Outcome	Most Likely	Somewhat Likely	Least Likely	Test of No Interaction[1]
Academic preparation:				
Grade point average	(0.03)	0.02	0.29 **	*
Credits earned in science	0.00	0.33 **	0.10	
Credits earned in math	0.06	0.19	0.18 *	
Credits earned in English	(0.05)	0.51 **	0.20	
Credits earned in foreign languages	(0.04)	0.27 *	0.10	
Credits earned in social studies	0.01	0.36 *	0.25	
Total academic credits earned	(0.01)	1.64 *	0.84	
Credits earned in vocational education	0.02	0.36 **	0.30	
Credits earned in remedial math	0.08 **	0.02	(0.13)	**
Educational expectations and attitudes:				
Students'	0.35 **	(0.09)	0.69 *	*
Fathers'	(0.14)	0.72 **	0.27	
Mothers'	0.00	0.58 **	(0.15)	
Self-esteem	0.02	(0.03)	0.03	
Locus of control	0.03	(0.08)	(0.03)	
How often talked with parents:				
About courses	(0.13)	0.02	(0.02)	
About school activities	(0.21)	(0.10)	0.21	
About studies	(0.27)	0.10 *	0.16	
About grades	(0.14)	0.04	0.17 *	**
About transferring to another school	0.09	0.07	0.21 **	
About taking or preparing for the ACT/SAT	(0.11)	(0.02)	(0.05)	
About college	(0.03)	0.03	(0.06)	
How often parents:				
Checked on homework	(0.05)	0.28 *	0.31	*
Helped with homework	0.00	0.35 *	0.37	
Gave special privileges for good grades	0.01	(0.06)	0.12	
Limited privileges because of poor grades	(0.10)	(0.14)	0.62 **	**
Required chores around the house	(0.06)	(0.10)	0.18	
Limited time watching TV or playing video games	(0.06)	0.04	(0.28)	
Limited time with friends	(0.15)	(0.07)	0.04	
Attended a school meeting	0.02	0.05	0.08	
Spoke with teachers	0.01	(0.01)	(0.01)	
Visited classes	(0.06)	(0.09)	(0.15)	
Attended a school event	(0.09)	(0.07)	(0.19)	
Misbehavior:				
Times late for school	0.42 **	0.41 **	1.11 **	
Times skipped classes	(0.18)	0.14	0.70	
Times missed a day of school	0.16	0.07	0.53 *	
Times in trouble for not following school rules	0.15	0.08	0.38	
Times put on in-school suspension	0.06	(0.06)	(0.05)	
Times suspended	(0.04)	0.05	(0.07)	
Times transferred for disciplinary reasons	(0.01)	0.02	0.02 *	
Times arrested	(0.03)	0.03	0.03	
Times spent time in juvenile home	0.06	(0.07)	(0.05)	

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

TABLE G.7

UPWARD BOUND IMPACTS, BY RANDOMIZATION DATE

Outcome	All Students: Randomized Before 8/93	All Students: Randomized 8/93 or Later	All Students: Test of No Interaction[1]	Participants: Randomized Before 8/93	Participants: Randomized 8/93 or Later
Academic preparation:					
Grade point average	0.08	(0.03)		0.10	(0.04)
Credits earned in science	0.21 **	0.05		0.26 **	0.06
Credits earned in math	0.20 **	0.00		0.25 **	0.00
Credits earned in English	0.22 **	0.14 **		0.28 **	0.18 **
Credits earned in foreign languages	(0.04)	0.25 *		(0.05)	0.31 *
Credits earned in social studies	0.15 *	0.16 **		0.19 *	0.20 **
Total academic credits earned	0.71 **	0.60 **		0.89 **	0.75 **
Credits earned in vocational education	0.26	0.08		0.33	0.10
Credits earned in remedial math	0.03	0.02		0.04	0.03
Educational expectations and attitudes:					
Students'	0.36	(0.08)		0.45	(0.10)
Fathers'	0.35 **	0.16 *		0.44 **	0.20 *
Mothers'	0.10	0.48 *		0.13	0.60 *
Self-esteem	(0.01)	(0.01)		(0.01)	(0.01)
Locus of control	0.01	(0.08)	*	0.01	(0.10)
How often talked with parents:					
About courses	(0.09)	(0.01)		(0.11)	(0.01)
About school activities	(0.07)	(0.22)		(0.09)	(0.28)
About studies	(0.09)	(0.06)		(0.11)	(0.07)
About grades	(0.08)	0.04		(0.10)	0.05
About transferring to another school	0.03	0.20 *		0.04	0.25 *
About taking or preparing for the ACT/SAT	(0.12)	0.02		(0.15)	0.02
About college	(0.03)	0.02		(0.04)	0.03
How often parents:					
Checked on homework	0.15	0.09		0.19	0.11
Helped with homework	0.19	0.17 **		0.24	0.21 **
Gave special privileges for good grades	0.05	(0.10)		0.06	(0.13)
Limited privileges because of poor grades	(0.05)	(0.09)		(0.06)	(0.11)
Required chores around the house	(0.07)	(0.03)		(0.09)	(0.04)
Limited time watching TV or playing video games	(0.01)	(0.09)		(0.01)	(0.11)
Limited time with friends	(0.14)	(0.05)		(0.18)	(0.06)
Attended a school meeting	0.04	0.03		0.05	0.04
Spoke with teachers	0.05	(0.07)		0.06	(0.09)
Visited classes	(0.13)	(0.02)	*	(0.16)	(0.03)
Attended a school event	(0.07)	(0.13)		(0.09)	(0.16)
Misbehavior:					
Times late for school	0.57 **	0.35 **		0.71 **	0.44 **
Times skipped classes	0.19 *	(0.19)		0.24 *	(0.24)
Times missed a day of school	0.28 **	(0.08)		0.35 **	(0.10)
Times in trouble for not following school rules	0.07	0.23 **		0.09	0.29 **
Times put on in-school suspension	(0.08)	0.13		(0.10)	0.16
Times suspended	(0.05)	0.07		(0.06)	0.09
Times transferred for disciplinary reasons	0.00	0.01		0.00	0.01
Times arrested	(0.01)	0.02		(0.01)	0.03
Times spent time in juvenile home	(0.02)	0.01		(0.03)	0.01

* Significant at the .10 level.
** Significant at the .05 level.

[1] Before considering tests about specific subgroup impacts, we first test whether there are differences in impacts between subgroups. If we reject the hypothesis of no difference, we then turn our attention to tests of each of the subgroup impacts.

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TECHNICAL APPENDIX H

DISTRIBUTION OF PROJECT-SPECIFIC IMPACTS FOR SELECTED OUTCOMES

This analysis focuses on three outcomes: (1) students' educational expectations, (2) course taking, and (3) grade point average. When looking at the average impact for all Upward Bound programs we ignored distinctions among projects and pooled the Upward Bound students into a single treatment group and the members of each of the control groups into a single control group, and compared the average gains for the two groups.[1] When examining variation in project impacts, we must think of having a treatment group, a control group, and an estimated impact for each of the projects in the sample and linking the impacts with a set of project characteristics.[2]

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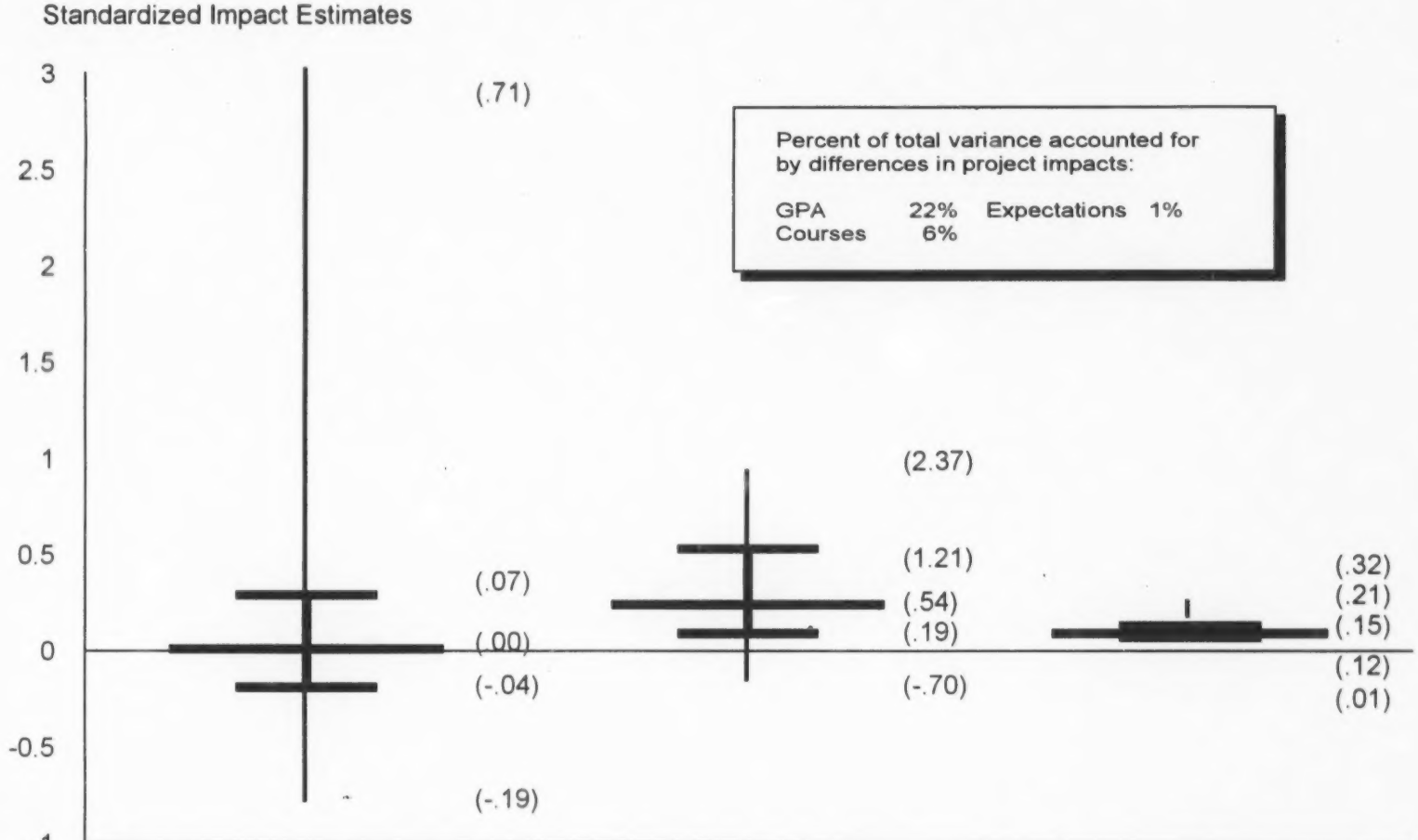
[1]Projects do not contribute equally to the average impact; instead, their contribution to the average impact is a function of the probability with which they were selected from the population of Upward Bound projects.

[2]Estimation of variability in project impacts and the relationship between impacts and project characteristics is undertaken using a hierarchical linear models (HLM) approach (see, for example, Bryk and Raudenbush 1992). Estimates of program impact reported in this chapter may differ somewhat from the average impacts presented in Chapter III. For example, the overall analysis that compared the mean for the Upward Bound group with the mean for the control group showed a difference of .8 credits. This is an unbiased estimate of the average impact of Upward Bound on students selected for the program. The HLM approach allows for variability around the average gain for the control group and the treatment group. Alternative estimates result from using different model specifications and their attendant assumptions. With different model specifications estimates ranging from .8 to .5 are computed using the HLM approach. The estimate of .8 is based on a model where we assume no random variability around the average for the treatment group. Allowing for random variability around both the control group mean and the treatment mean produces an impact of .5 credits.

FIGURE H.1
DISTRIBUTION OF IMPACTS



Metric estimates presented in parentheses. Estimates differ from those presented in Chapter III because of differences in model assumptions used to examine variation in project impacts.

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We find very different results when looking at variability in project impacts for students' educational expectations, grade point average, and academic course taking (see Figure H.1).[3,4,5] Impacts on students' educational expectations are tightly clustered around the national average, while there is considerable spread for grades and course taking. For educational expectations we find that half the projects have an impact that falls within a range of less than one-tenth of a standard deviation. For grade point average and credits earned, half the Upward Bound projects fall within a range of about .5 standard deviations.[6] Below, we more closely explore the impact distributions for grade point average and course taking.[7]

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[3]Estimates of projects' impacts are empirical Bayes estimates and adjust for the uncertainty in estimating impacts in small samples. Empirical Bayes estimates are based on a weighted average of the project's estimated impact (difference-of-means) and the average impact for the sample. More weight is given to the project impact when it is precisely measured and less weight is given when the project impact is measured with less precision.

[4]Figure H.1 shows three distributions of impacts. The wide horizontal line for each distribution is the second quartile, which represents the median (50 percent of the projects have impacts below the median point for the distribution); the horizontal line directly below the median corresponds to the first quartile; and the horizontal line directly above the median is the third quartile (25 percent of the projects are below the first quartile, 75 percent of the projects are below the third quartile, and 50 percent of the projects are located between the first and third quartiles). The vertical lines above and below the quartiles show the range of the distribution.

[5]For purposes of comparing the variability in impacts across the three outcomes, we use both standardized measures of impacts and impacts expressed in their natural metric. The standardized impacts are simply the impact estimates divided by the population standard deviation for the outcome. The standardized estimate shows the relative impact per standard deviation. Using the natural metric tells how much effect Upward Bound has on a variable in terms of the units in which was measured. For example, for educational expectations the natural metric is years of schooling. The impact estimate tells us how many more years of schooling students selected for Upward Bound plan to complete than members of the control group (.11). The standardized measure tells us the treatment group and control group are .17 standard deviations apart.

[6]Grades are reported on the following scale: 0 = F, 1 = D, 2 = C, 3 = B, and 4 = A.

[7]Besides examining the direct variability in impacts, we can ask whether the differences among projects in impacts contribute much to the total variability in students' scores for these outcomes. We can estimate this contribution by computing the proportion of the total variability accounted for by differences among projects. Using this measure, we find that less than 1 percent of the total variance

(continued...)

Grade Point Average

- The findings for grades show that when potential differences among projects are ignored, Upward Bound participants and members of the control group had similar experiences (that is, there is no impact, on average). However, Figure H.1 shows that in some projects participants fare much better than members of the control group. In fact, about 25 percent of Upward Bound projects have a moderately large impact of .07 (.3 standard deviations) or greater. This means that for about 100 of the 395 Upward Bound projects nationwide, participants had grade point averages at least .07 units above what they would have achieved outside Upward Bound.[8] For almost half of those 125 projects, Upward Bound participants' grade point averages were raised by .1 to .2 units.[9]

- The median displayed in Figure H.1 implies that for about 50 percent of the projects there is no impact or a negative impact. A possible explanation for a decline in grade point average may be that some students are taking more challenging courses. To explore this possibility, we looked at impacts that adjusted for differences in the number of academic courses students took between the end of 9th grade and the end of the 1994 school year. We discovered that students who earned more academic credits had higher grades. This finding may result from more motivated and academically prepared students attempting and earning credits in academically challenging courses and these same students having higher grades than other, less motivated and less well-prepared students. Given the positive relationship between grades and course taking, it is unclear why we find no impact for Upward Bound on students' grade point average. We will investigate this finding further after we analyze the second follow-up data.

Course Taking

- The average impact of Upward Bound on academic course taking is positive and significant. Looking at the full distribution of impacts (not shown) shows that about 85 percent of the projects have a positive impact and about 15 percent had either no impact or a negative impact. Furthermore, for a quarter of the projects we find that members of Upward Bound earned 1.2 or more additional academic credits than members of the

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[7](...continued)
in educational expectations can be explained by differences in project impacts. For grades and course taking 22 percent and 6 percent of the total variance is accounted for by differences in project effectiveness, respectively.

[8]We cite 395 here as the total number of projects because the universe for the sample used for the longitudinal impact study includes only projects hosted by two- and four-year colleges located in the 50 states and the District of Columbia. A detailed discussion may be found in Technical Appendix A.

[9]This is based on the visual inspection of the full cumulative frequency distribution for impacts, which is not shown. Figure H.1 summarizes that distribution in terms of its quartile and extremes.

control group. This means that within these projects, on average, members of Upward Bound completed at least one more academic course than students in the control group.

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